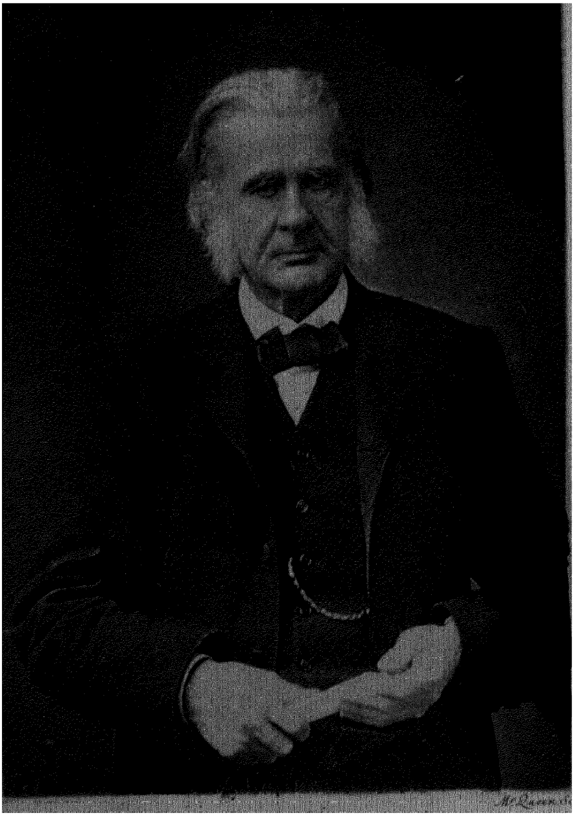
SIXTY YEARS OF BOTANY IN BRITAIN



T. H. Huxley.

From a photograph by Downey, 1890.

SIXTY YEARS
OF
BOTANY IN BRITAIN
(1875—1935)

IMPRESSIONS OF AN EYE-WITNESS

BY

F. O. BOWER, Sc.D., LL.D., F.R.S.

EMERITUS PROFESSOR OF BOTANY IN THE UNIVERSITY OF GLASGOW

WITH FRONTISPIECE
AND 13 OTHER ILLUSTRATIONS

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PREFACE

Few of those who now pass easily from schools where Science is taught into the universities, and onwards to graduation, have any idea of the obstacles that faced students of an earlier time. The opening chapters of this book tell, for the particular Science of Botany, how rough was the road for anyone whose scientific instincts were stirred before schools and universities were ready to deal with his special needs. This is here illustrated in the case of an average boy born four years before the publication of the *Origin of Species*, who entered a Public School in 1868, and went up to Cambridge in 1874.

While he was still at school Mr. Forster's Education Act of 1870 was beginning to take effect. It ordained that the teaching of Science should be introduced into the curriculum of the national schools throughout the country. Consequently there arose a sudden demand for the supply of duly qualified teachers, which the universities were unprepared to meet. This led to the foundation of the Normal School of Science at South Kensington: and a very strong staff was appointed there to teach them, with Professor T. H. Huxley as its Dean.

Prior to this event the Natural Sciences had been taught orally rather than practically in the universities and colleges of Britain. Huxley's own way of teaching was itself a recall to the true way of learning, and it re-vitalised academic Biology. Each student in his laboratories became, by his heuristic method, not only a hearer of the story of past achievement, but also an active agent in the pursuit of knowledge: and research naturally followed. The author, as a budding botanist at Cambridge, was privileged

to witness and even to take a minor part in promoting this essential change of method. Through the death of his contemporaries he is now the only botanist left who can tell the tale as an actual participant. He here describes the modest start made by Thiselton-Dyer, in applying Huxley's method to the Science of Botany: the activity that was soon aroused in the laboratories at South Kensington, and its subsequent spread thence throughout the country.

This new stimulus has retained its force through more than sixty years. The personal sense of participation in a nascent cause may have faded, but its effect must surely be permanent. For during that period the demands of modern life have expanded, and the results of research in Pure Science have found ever increasing application in the daily experience of us all. Such results tend to impress the stamp of sustained vitality upon that expansion in the study of the Biological Sciences in Britain, which originated at South Kensington in the 'seventies of the last century.

The text has been enlivened by the aid of a few carefully selected portraits. The author wishes to express his thanks to those who have helped him to that end. To Messrs. Macmillan for the loan of the frontispiece: to the executors of Sir Joseph Hooker for the use of his portrait, together with that of his colleague, Mr. Bentham: to Lady Thiselton-Dyer for the photograph of Sir William: to Lady Balfour for the portrait of Sir Isaac Bayley Balfour: to Professor F. W. Oliver for the use of the block representing his father: and to Dame Helen Gwynne-Vaughan and Mrs. Kidston, for their permission to reproduce the vivid photograph of the authors of *The Fossil Osmundaceæ*. Also to the Royal and Linnaean Societies for the loan of portraits from their respective libraries, and to Professor Walton for suggesting the use of the block of Professor Williamson. These portraits for the most part present the originals as they were about the period to which this story relates.

Finally, the author acknowledges with gratitude permission

from the delegates of the Oxford Press to reproduce in Chapter VIII an original letter dated January 1887, and addressed to them by Sir Isaac Bayley Balfour and Professor Vines. In it was conveyed the proposal to found a new journal which, under the title of the *Annals of Botany*, has now completed, under the auspices of the Press, fifty years of useful service to Botanical Science.

F. O. B.

RIPON,
January, 1938.

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HOME

On a summer's day in the early 'sixties of the last century two small boys might have been seen crossing the southward-sloping fields of Elmcrofts, Ripon, where in 1855 the eye-witness had been born: they would have been making for a flat area of meadow-land called 'Ripon Ings'. These fields were not exactly water-meadows, as that name would seem to imply: they were drained, but still moist enough to ensure a heavy crop of hay in the dryest summers. The goal of the boys was a small pond, without any spring to feed it: but clay-puddled like a dew-pond, and rarely if ever dry. It lay between two fields, and a rail continuous with the dividing fence crossed it diametrically, so that the pond served for watering the cattle in both. Along this rail the boys used to climb, and with improvised nets and bottles they would secure and examine specimens of the pond-life that thronged the stagnant water. Like Adam they knew the various types by names devised by themselves: but they never advanced further than noting the external appearance and behaviour of their prey. It was a most rudimentary essay in the study of organic life, though the spirit of enquiry was there. The boys were the brother of the author, and the 'eye-witness' himself.

Again in the early 'sixties another line of interest was suggested to these boys by the gift of a box of chemicals from an enlightened aunt. Some simple printed directions as to their use were supplied with the box. Naturally the boys threw down precipitates, produced smells and coloured flames, and changed the tints of solutions: they even demonstrated these wonders to the assembled

household. But their efforts were in the direction of surprise and amusement rather than of any coherent scheme. Directive influence was wanting, though quite possibly it might have been supplied by some practising chemist in the town. It would be fruitless to speculate what might have happened if any qualified guidance had been at hand for these potential naturalists. The average English home of the 'sixties encouraged any signs of taste for the arts, but the day of methodical teaching of the sciences had not yet dawned there, except on the obvious fringes of natural history, particularly in study of a local flora.

In this last both stimulus and guidance came to us in 1862, by the gift of a copy of *Rambles in Search of Wild Flowers*, written by Miss Margaret Plues, daughter of the headmaster of Ripon Grammar School. I remember to this day the rather crudely coloured floral plates of her book. But for me, the youngest of the family, the text offered little attraction. Moreover the governess of the period had no co-ordinated knowledge of such things, though she did her best to direct us towards the natural objects of the country. My father, who was a county magistrate, had some interest in science, but no training therein. Volumes of Buffon, Humboldt, and Liebig: also of Lardner and Gosse, were, however, on his shelves. On the other hand, my mother's eldest brother, the Rev. F. O. Morris, Rector of Nunburnholme, was the author of well-known books on British birds, butterflies, and moths, which found their place in the libraries of many country houses of the time. He was a naturalist of an early Victorian type, and being sixty years of age when the *Origin of Species* was published, it was natural that he should retain to the end of his life his biblical belief in special creation. It was doubtless from him that the expression 'Darwin, Huxley, and the Atheists' passed into the conversation heard in my early years at home: but I do not myself remember to have ever entertained any anti-evolutionary bias.

Notwithstanding the restrictions of the Victorian Age it was a happy and wholesome life we led at Elmcrofts in the 'sixties, with

ponies to ride and all the usual amenities and supplies of garden, plantation, and field. Meanwhile in the school-room the foundations were being laid for ordinary education. But then the curtain fell. Living henceforth by school and college terms, I never again saw my home in the full glory of early summer blossoming, save as an occasional visitor. The freedom of home life was exchanged for the stark standardisation of a junior school of Victorian times. Individuality was sacrificed to arithmetic, Latin, and French, with a beginning of Greek preparatory for entry at a public school. Observation of Nature was temporarily swamped, though I do remember some spirited earwig-racing. For on our country crocodile-walks we boys discovered that in autumn earwigs found a lodging in the hollow stems of the beaked parsley. These were gathered from the hedges, and brought back. The course was laid out on a table. If the competitors were detained by a finger laid on their rear forceps a good start might be made, while the track was indicated for each by a pencil as a moveable obstacle. This was the only natural history experiment I remember during the period of the preparatory school.

Seventy years have brought changes in scholastic method. The general tendency has been to relax the procrustean rule that all must fit into one rigid mould: and that it is the worse for those who do not. A certain degree of uniformity of curriculum there must always be: and it is specially necessary in the earliest phases of education. As these are passed an ever increasing latitude of choice is now offered. The boy with an idiosyncrasy is no longer treated as an oddity to be moulded to a uniform model whether he will or not. He is given his opportunity, and encouraged in his solecism provided it be a reasonable one. We who belonged to an earlier time can realise on the one hand what we have lost by the uniformity of that model: but on the other hand we see more clearly now, and appreciate more warmly those opportunities and touches of encouragement which softened for us the angles and barriers of a rigid system, at a time when such elasticity was the exception rather than the rule.

CHAPTER II

REPTON SCHOOL 1868–1874

Repton School, which I entered in 1868, is one of those sixteenth-century foundations that expanded in the nineteenth upon a new scale of growth. It is fortunate in the natural beauty and in the history that distinguished the spot long before the will of Sir John Porte in 1556 created the school. A traveller going south from Derby sees from afar, rising among green pastures, the tall and slender spire of Repton Church, perpendicular in style. Crossing by a stone bridge over a curve in the river Trent, and further on by a culvert over a backwater of the stream's former course, he will pass uphill almost under the shadow of the church and then, turning left, he will enter the wide school yard through a Gothic arch. This formed part of the gate house to the Augustinian priory whose remains underlie and still form a notable element in the school buildings. Every day of his school life the Reptonian traverses this spot, from which it is possible to see in one glance stonework dating back at least to the tenth century. In the remote age of the Heptarchy here stood the capital of Mercia, but of this there are no visible remains: on the other hand, certain strange spiral pillars of a Saxon crypt, which can be dimly seen by boys from the school yard, indicate beneath the chancel of the church the traces of a very early shrine. A large block of buildings of the ancient priory still stands, adjoining the school yard: to-day it forms the focus for most of the school's activity. The Norman pillars of its 'Undercroft'—a chamber now divided between the school museum and the masters' common room—mark a date later than the crypt. The Pears Memorial Hall is itself built upon

the site of the priory church, whereof a few shreds of masonry are still preserved. Over all rises like a needle the slender spire of the parish church. The very stones of Repton not only reveal beauty and antiquity concentrated in the midland plateau, but also suggest an atmosphere of vigorous life and learning, consciously or unconsciously present among generations of boys, as a true *genius loci*. With a chronicle of the 'humanities' written thus plainly for all to see, and illustrated in later time by a good repute for orthodox successes at the universities, it is not surprising that Repton should have been slow to relax its time-tables, and to admit the methodical teaching of natural science into its curriculum.

In the middle of the nineteenth century the public schools of this country were generally standardised on the model of Dr. Arnold. The life was robust and in some respects Spartan, while the balance between work and games was evenly held. The curriculum was mainly classical and mathematical, with a modicum of modern languages, and a smaller modicum of history and geography. The development of a modern side was in its infancy, or non-existent: while science was denied even a modest place, being crowded out by such subjects as Latin verse, then deemed more weighty. There was, in fact, little differentiation in the milling process to which the raw material of new boys was subjected. Boys were just boys, and their education was dealt out to them all with no very discriminating hand. Repton was not one of the first of such schools to include science in its scope: nevertheless during my sojourn there opportunity was not wholly denied to those whose inclination tended to the study of Nature. It is to be remembered, however, that it was only in 1866 that the question of teaching science in public schools had first been openly raised. In that year the British Association met in Nottingham. Huxley, who presided over the biological section, pointed out the deficiency in their curriculum: but he laid the blame at the door of the universities, asserting from the chair that science would never be taught in schools until the universities themselves recognised its

place more fully as a feature in a liberal education. This prepared the way for a paper by Dean Farrar, based upon his own experience as a master at Harrow. He suggested the substitution of elementary science for the making of Latin verse. But on the other side the difficulty was urged that duly qualified teachers were not available. The public schools looked to the universities for the supply of masters prepared to teach elementary science: at that time they looked in vain.

Many years elapsed at Repton before laboratories were established with a duly qualified teaching staff. But before I went there in 1868 the earliest steps towards meeting the growing call of the natural sciences had already been taken by Dr. Pears, a prince of headmasters, as he was described by so good a judge as Dr. Vaughan, himself headmaster of Harrow. In the early 'sixties Dr. Pears had encouraged the collection of butterflies, moths, and beetles by the boys in the country round Repton, by offering prizes for competition. A knowledge of the local flora had also been stimulated by prizes for herbaria: and in 1866, with the aid of the local doctor, a very creditable *Flora Repandunensis* was published, while successive editions followed to which the boys contributed many records of species and localities. The eye-witness himself possessed before he left school a British herbarium of over 600 species. In 1866 Dr. Pears had founded a scientific society at Repton. Its first meeting, as recorded in the *Reptonian* magazine, was held on March 13, some six months before the question of the teaching of science in schools was raised by Dean Farrar, at the British Association at Nottingham. The society was supplied with a compact and well-selected library, to which I was indebted for my first introduction to certain scientific classics of the time. Dr. Pears also organised an annual soirée for the exhibition of specimens, and the delivery of lectures by the boys themselves. Occasionally during the winter months, from 1867 onwards, short courses of evening lectures would be given by some scientific expert. However stimulating such isolated events might be, they

were all outside the prescribed curriculum, and could be no real substitute for organised study.

In *Tom Brown's School-Days*, that classic picture of public school life in Dr. Arnold's time, a vivid personality is seen in Martin, a born naturalist, nicknamed 'the Madman'. He fitted ill into the rigid mould of a classical school, though Dr. Arnold himself sympathised with his unconventionalities. A generation later the same type, though softened by the conditions of the time, appeared at Repton in the person of Frank Penrose, who eventually became a member of the senior staff of St. George's Hospital. Tall, lithe, with a cut of clothes specially suited for tree-climbing, he knew birds by instinct. In the nesting season he established a freemasonry with the keepers at Foremarke and Bretby by taking their hawk's nests, while they gave him the freedom of their woods. His tastes as a naturalist were winked at or even encouraged by an understanding house-master. A group of us in 'the Hall' were also keen bird's-nesters. We were not over particular about observing bounds. Some of our escapades, zoological or botanical, were not unlike those of Martin's field parties at Rugby. One of these may bear the telling, trivial though it was. Three of us were bird's-nesting out of bounds, when a truculent farmer suddenly appeared, and snatched off my straw hat for the purpose of identification. A short time before when the subject of the weekly English essay which each boy was called upon to write had been given out, I had scribbled it in the crown of my hat: 'The Drama'. Assuming that it was my name, the farmer said triumphantly, 'Now I've got yer, Misther Thomas Dhrama'. I saw no cause for enlightening him, and naturally I heard no more of it. I do not, however, think it would have gone ill with me if it had ended otherwise; for the headmaster knew well the difference between a budding naturalist and a real offender. He would often release us from calls and bounds, as an encouragement to our collecting.

As an example of the personal stimulus of Dr. Pears, and its

effect, I may describe an experience of my own. A prize was offered annually for arithmetic, and it was open to the whole school. But the examination was held on a holiday afternoon, a fact that narrowed the field. In 1873, I entered for a sporting chance, and to my surprise headed the list. My natural turn towards botany was already known to Dr. Pears, and one of the prize volumes was Figuier's *Vegetable World.* At the soirée of the scientific society in the following winter I gave a short lecture, based frankly in portions of that book, and illustrated by enlarged crayon copies of some of its beautiful French woodcuts. However jejune this performance may have been, the tactful insight of the headmaster had stimulated my first essay in public speaking on botany. Another example of Dr. Pears's encouragement of scientific interest among his boys was the establishment of a set of meteorological instruments in the school-yard, in view of everybody. They were placed under the charge of W. M. Conway—who afterwards became the late Lord Conway of Allington. For a long period he made daily observations and recorded them on the meteorological charts of the time. I remember an occasion when the speaker at one of the meetings of our scientific society had failed, how Conway at very short notice gave an excellent account of the use of those charts, if duly filled up and sent to the meteorological office. Thus early he began to fit himself for taking observations, an invaluable practice for his later travels and mountaineering.

About the year 1872 the same enlightened aunt who had given us the chemical chest aided practical demonstration by the gift of a microscope. This instrument, at that time as unusual in a boy's study as was the astronomical telescope efficiently used in an adjoining study by Conway and R. J. Ryle, transmuted into actual observation some of the details described in the botanical books available to us in the library of the scientific society. Our methods of preparation were extremely crude: nevertheless we were able to examine various types of starch, while structurally we observed

the superficial stomata and hairs, and internally the wood-vessels of spiral and pitted types, and crystals of calcium oxalate. A considerable collection of slides was made, though owing to our imperfect knowledge of mounting media they quickly deteriorated. The want of skilled guidance was the real snag. The will to observe was present with us, but how to perform we found not. All this may seem trivial now, but when placed in the setting of 1866 to 1874 the facts show how an alert headmaster kept alive such sparks of intelligence as appeared in individual boys, at a time when scientific teaching was not yet formulated in public schools. Moreover well selected books of the period were made available to us, such as Tyndall's *Heat, Light, and Sound*: or Carpenter's *Microscope*, the *Micrographical Dictionary*, Figuier's *Vegetable World*, Bentham's *Handbook of the British Flora*, and even the twelve expensive volumes of Sowerby's *English Botany*. Thus for us a corner of the veil of Nature was lifted, and inner penetralia with attendant mysteries suggested to the young imagination.

It was not till the early 'seventies that an hour a week was assigned to science in the regular curriculum at Repton. Well chosen text-books were adopted: such as Green's *Physical Geography*, Page's *Geology*, and Huxley's *Physiology*. But here came in the difficulty that none of the masters of the time were trained in any of these sciences. They did their best in their struggle with unfamiliar subjects: and under their guidance we learned from Green at least something of the world we live in, and from Huxley a general knowledge of the skeleton and various organs of the human body, and their functions. But geology was a harder nut to crack. Page's textbook was assigned to the upper fifth form, then taught painfully but well by an elderly classic, who had apparently no practical knowledge of the earth's crust, or of the fossils that it contained. He probably smiled inwardly as he turned the geological lesson into channels more familiar to himself. The afternoon before the event he would announce: 'To-morrow we shall take Chapter "X" of Page's *Geology*, and each of you will

bring written out—Greek or Latin and English—all the names and technical terms contained in it, with their meanings'. The wily old fox had literally gone to ground in his own familiar earth of the classics. But the odd thing was that for me his method worked. I remember to this day Page's illustrations representing animals of the Devonian period, and the meaning of the names attached to them, such as *Pterichthys* and *Coccosteus.*

During my last year at Repton Dr. Pears retired, and with a new headmaster, who soon discovered my bent, I felt more freedom to indulge it. Many an hour did I spend in the sixth form reading the botanical parts of Carpenter's book on the microscope: meanwhile the classical work of the form would be going forward, to which I rendered only the slightest lip-service. On the other hand, on leaving school I was fairly well acquainted with the life-cycle of mosses and ferns: for the astonishing results of Hofmeister had already filtered into Carpenter's text. These primitive land-plants fascinated me thus early, though at the time I had but a dim idea of their place in Nature. It was during my last term at Repton that I made up my mind that botany should be my life's study. I remember sitting down on a heap of stones by the roadside and thinking out that resolution, though without any clear conception of what it might involve. I had no insight into physics, chemistry, or geology, which form the foundation upon which the study of a biological science must needs rest: nor was there any one at hand qualified to advise. I left school with this blind determination, insecurely based upon such general education as an ordinary sixth form boy of the period would possess. It may interest others as a problem of education to consider whether it is better to hold a resolution such as mine, without knowledge whither it may lead: or to defer such decisions till the formative years that follow on leaving school. Much will depend upon the individual, and the strength of his convictions: but possibly more upon such directive advice and help as the university may provide. As we shall see, the eye-witness went up to Cambridge, entering at Trinity, the col-

lege from which in the seventeenth century John Ray, through his *Historia plantarum*, had led Europe towards a natural system of plants. His bust by Roubiliac is treasured in Wren's Library of the College: and in its Hall a learned biographical oration was recently given by Sir Albert Seward, commemorating the centenary of the Ray Society founded in his honour. But in 1874 I found no evidence that his botanical spirit was active there.

CHAPTER III

CAMBRIDGE 1874–1879

I entered Cambridge as a pensioner of Trinity College in October 1874. At the end of my first term I was free from the routine examinations of the time, and was eager to begin a methodical course with a view to the Natural Sciences Tripos, in which my chosen subject of botany would be the spear-head. But, as I found it in the spring of 1875, Cambridge was not an inspiring field for a beginner in those sciences. It has been explained how the teaching of science in a public school was not yet methodised, and Cambridge did not lay itself out to fill the gap. So I, who like others of the time needed a thorough grounding in physics and chemistry, fell between two stools, with results that have dogged my steps ever since. I was advised by my tutor to attend a course in elementary physics, given by a fellow of the college. The lectures were purely forensic, not being aided by illustrations of any sort. The insufficiency of the course was dimly felt at the time by a disappointed listener. This youthful judgment was confirmed in later years, when he became aware of what such an elementary course might have been, if aided by well-chosen demonstrations. The classes in chemistry in 1875 were more satisfactory. The professor's lectures and the elementary laboratory were attended: but they formed no coherent whole. Moreover in the laboratory the individual student was thrown among scores of others in a similar position to himself. The demonstrators did their best to give individual attention to elementary students who were all doing different things: but certainly the result on an average student was a very deficient equipment in this subject also.

An aspirant in botany would naturally look round for special guidance in that science, for which a chair had existed in Cambridge from 1724. Its occupant in the 'seventies of last century offered in the calendar an elementary course of lectures chiefly on descriptive botany, but during the Easter term only. I sampled those lectures and found them wanting both in spirit and in substance. The calendar also announced 'herborizing excursions, should the circumstances permit': as far as I was aware the circumstances never did permit. There was no suggestion of organised work during the winter, nor yet of any laboratory practice. In 1875 the official teaching of botany in Cambridge University was moribund in the summer, and actually dead during the winter. It was different, however, with the class in elementary biology instituted by Dr. Michael Foster, then Trinity Prælector in Physiology. It was conducted after the method introduced in 1872 at South Kensington by Professor Huxley, and his book was used as the text, while assistance was given in the laboratory by a full corps of willing demonstrators. The daily introductory lecture, and the detailed personal help given in the laboratory-class that followed it, formed a coherent whole: they were a revelation to me. For the first time in that summer term of 1875 I learned what it meant to be taught science in a rational way. Moreover the methods of preparation and treatment of living material for immediate observation, and of permanent mounting in transparent media opened for me a new arena of microscopic technique. This course counterbalanced the other disappointments: it launched my cockle-shell boat on the tide of biological enquiry, for the inwardness of the Huxleyan method lay in the fact that each student became himself an investigator at first hand.

There was, however, another circumstance that made the year 1875 notable for me. This was the publication of Sachs's text-book in English. Its translation was carried out by Dr. A. W. Bennett, with general supervision and editorial notes added by W. T. Thiselton-Dyer. Both Britain and America had hitherto been

served by text-books good in themselves, but devoted chiefly to the morphological and systematic treatment of the higher plants. They had failed to bring into due balance those advances in physiology, and in the life-histories of the lower organisms which had been made on the continent, and particularly in Germany. No British text-book had offered a philosophical digest of the whole science, such as that presented by the text-book of Sachs. To me this volume supplied exactly what was needed as a sequel to my own reading. The work was particularly well illustrated, and its ample references to the latest literature of the science opened a new vista. I absorbed its text eagerly, and prepared myself to verify the contents so far as possible by personal observation.

A third, and for me personally a more important circumstance, was making acquaintance early in 1876 with S. H. Vines, who was beginning, as a newly elected Fellow of Christ's College, to lecture there. Hitherto the only 'College' lectures available in botany at Cambridge had been those given by a Fellow of Sydney Sussex College. I attended them in 1875; but they proved to be founded on Hofmeister's *Higher Cryptogamia*, and as there were no illustrations nor specimens they were less intelligible than reading that brilliant book itself. Vines as a lecturer was very different from this. Fresh from B.Sc. Honours in London, and a First Class in the Cambridge Tripos, and having had experience of Huxley's laboratory at South Kensington, it was not merely book-learning that he gave. He appeared to me as a vital personality, revealing contacts with living organisms and their functions. His lectures together with Sachs's text-book supplied much that had before been lacking. One thing more was needed: that was a botanical laboratory where under guidance a student could himself work practically. This, owing to Vines's own initiative and Dr. Foster's hospitality by lending a room in the new physiological department, became available in the autumn of 1876. Thus as a third-year undergraduate I at last found myself supplied with what I had set out to seek at Cambridge: viz. an opportunity for the study

of botany practically, and under expert tuition. But two priceless years had been spent in the search for it: nor was this completely satisfying. As will shortly appear, I took an early opportunity, on the suggestion of Vines, for seeking further help in a foreign university.

The natural result of the disappointments and delays of my earlier years of residence at Cambridge appeared in the examination for the only scholarship specifically assigned for the natural sciences at Trinity in 1877. It was open to members of all colleges, and to all years of residence. I entered as a third-years man, ill prepared in all subjects but botany; and the result was what might have been expected. The award fell to a first-years man from another college, whose principal subject was chemistry. These facts seem to date with some degree of accuracy the introduction of methodical teaching of science into public schools, though the change was not synchronised in them all. A man in his third year at Cambridge in 1877 had left school too soon to take advantage of it: a first-years man coming up, duly equipped from school with elementary physics and chemistry, was able to give better examination-results.

After this followed for me the Natural Sciences Tripos. It was at that time divided into two parts: the first was then a qualifying examination, and the candidates were not classed: in the second part the list was classed. The result was a place with first class honours in botany.

It was in the interval between the two parts of the tripos that I paid my first visit to a foreign university. During eight weeks of a hot summer in Würzburg I learned laboratory methods directly from Professor Sachs.

In November 1877 the honorary degree of Doctor of Laws was conferred by Cambridge University upon Charles Darwin, in the Senate House, and I was present in the gallery. I saw the stuffed monkey suspended over the head of the graduand by cords from the galleries, and heard the public orator's address interrupted by

the humorous amendment, 'And please, sir, do not forget to tell us that he is very well descended'. But an incident in the Trinity Chapel impressed me more than the graduation ceremony. Darwin had been a guest in the college for the event, and he attended chapel in the evening. It gave an opportunity for seeing the great man at close hand, and I seized it. There is only one exit from the chapel, and I posted myself close by the door. Darwin passed out within a yard of me. It was the only time I ever saw him, but the memory confirms my appreciation of Collier's portrait hanging in the rooms of the Linnaean Society. The passing view, however, and the very spot where the incident happened intensified my impression of the man. For though Darwin was himself a member of Christ's College, he was thus seen in Trinity within a few yards of the statues of Newton and of Bacon. Such a collocation of these three great figures in science and philosophy, whether in effigy or in reality, could not fail in its effect upon a mind at a peculiarly receptive age. Darwin died in 1882, within five years of this event.

At the close of my residence at Cambridge the new teaching of the science of botany was still undifferentiated, though already tending to spread with added efficiency into applied channels, such as agriculture and plant-pathology. A loose relation with the former of these is illustrated by a visit to Rothamsted of a student-party from Cambridge, about the year 1879. It originated from the physiological department, following on an invitation from Sir John Lawes, who gave us luncheon in his charming Elizabethan house. We were conducted by Dr. Michael Foster, and the party was made up mostly of students in physiology and botany. At that time the Rothamsted station was the private property of Sir John Lawes. Many years elapsed before the foundation of the Trust under which the work of Lawes and Gilbert has been continued, and so greatly amplified. We were shown standing crops, particularly on the plot of permanent grass laid down twenty years before by Dr. Gilbert, who explained to us his method of recording the results. The question of the sources of nitrogen in legu-

minous crops was also brought before us, in relation to experiments dating from 1857: though its riddle was yet unsolved.

There is little more that need be told of my scientific experiences at Cambridge. The prospects there for a young graduate who had not even been a scholar of his college were not good. A small lectureship under the committee for the higher education of women in Cambridge gave practice in lecturing, while a partnership with Adam Sedgwick, the zoologist, in coaching for the First M.B. examination brought in a small financial return. Nevertheless two of my own private pupils in botany passed into the first class in the Tripos. During these years I took advantage of the laboratory which Vines had established, and a widening experience was thus gained which tended in some degree to make up for the previous loss of time. But it was clear by the summer of 1879 that no opening was to be expected in Cambridge. The best course would therefore be to gain further experience by again attending some foreign university. The choice of 1879 fell upon Strassburg, where Professor de Bary had gathered about him a cosmopolitan group of post-graduate students: moreover Vines was already at work there. So after the Easter term Cambridge, and in particular Trinity College, were finally left behind, though with affection and deep regret. But I could not help feeling a lingering sense that such official guidance in botany, as I had confidently expected in 1874, had not been found there. In point of fact Cambridge then shared the deficiencies of the time with other British universities. There as elsewhere the essential truth was only beginning to dawn upon the official representatives of the science, that a practical subject such as botany must be studied practically, and throughout the year: and that living organisms, including the simplest of them, should be treated from the point of view of their vitality.

I cannot close this brief account of my experiences at Cambridge without some reference to music, which next to science was my chief interest there. My brother and I had taken up re-

spectively the violin and violoncello from very early years. As small boys we had had the advantage of concerted playing of classical music at home: with Mr. Bates, the organist of Ripon Minster. We continued our music at Repton, being directly encouraged in this by Dr. Pears. With another boy-violinist, Chamier, we had played string trios by Corelli at more than one of the school concerts. At Cambridge in 1875 we found others interested in string music: with them we used to meet regularly after Hall to play quartets, my brother having taken up the viola, while the two violins were C. F. Abdy Williams and W. H. Blakesley. After some terms of practice together in the old court at Trinity, we conceived the idea of giving public concerts. Among a group of friends, chiefly from Trinity, the Hall, and King's, the scheme of weekly chamber concerts was floated: they soon became known as the 'Wednesday Pops'. They were given at 8 p.m., in the small room of the Guild Hall. The programmes were of one hour's duration, and usually consisted of two concerted works, with an interlude of vocal and instrumental solos between them. The University Musical Society, of which all the members of the string quartet and other performers were members, at first declined to take the risk of this new venture. So we went on independently, backed by a guarantee fund raised in Trinity, which however was never called upon: for after a season in the Lent Term of 1876 we found ourselves with a financial balance. This we offered to the society together with the 'goodwill', and it consented to continue the series as a new branch of its own activities. When the members of our string quartet graduated and went down, the scheme tended towards professional hands. But after vicissitudes of which I have only imperfect knowledge, it still survives in its original amateur form, as the Cambridge Musical Club.

There was little difficulty in making up the weekly programmes, for the string quartet had acquired a considerable repertory from the classical period. Moreover we were fortunate in the help of

able pianists and singers. Of the former Stanford, Rowe, J. T. N. Lee, Fuller-Maitland, W. Austen-Leigh, Dr. Garrett, G. F. Cobb, Barclay Squire, and others took part in concerted works, or appeared as soloists or as accompanists. As singers, either soloists or in vocal quartets, there were Penrose, the Lytteltons, T. H. Orpen, Vines, and others. No ladies appeared in our programmes. We were particularly indebted to Stanford, lately returned from his studies in Germany. He not only took part in many concerts, but also at times coached us. An incident for which he was responsible is worth relating. The evening before the Honorary Doctorate of Music was to be conferred on Joachim by the university happended to be a Wednesday. We had seen the great man dining with Stanford at the high table in Trinity, and went off to play our weekly programme, which ended with a pianoforte quintet by Raff. As we came down from the platform, after an indifferent performance, who should we find but Joachim himself, sitting on a chair at the stage door: behind him was Stanford, who said with a laugh, 'I had to bring him to hear how my boys could play'. Joachim was kindly, as always: 'You keep very good time', he said, thus touching the fundamental point of music, while overlooking our many lapses. The next day, at an orchestral concert, he played the Violin Concerto of Beethoven, as he alone could play it. I never hear it now without recalling the glowing spirit, and even many details of his masterly rendering of that greatest of all compositions for the violin.

CHAPTER IV

WANDERJAHRE

At the middle of the nineteenth century British botany was marked by strange contrasts. It witnessed the production by its seniors of descriptive works of fundamental importance, such as the *Species filicum*, and the *Genera plantarum*: overshadowing all was the *Origin of Species*. But up to 1875 the universities almost entirely failed to train students who as juniors should be preparing to take the places of the veteran authors. Moreover it was collecting, classifying, and recording that were the order of the day; while anatomy, physiology and the study of the complete life-cycle, especially in the lower forms, were given only a minor place in the university curricula, if indeed they were not wholly neglected. It seems strange now to look back upon the deadness of botanical teaching in the universities in the years prior to 1875, though this was a period of extreme brilliance of individual production: to read the *Life and Letters* of Darwin, Hooker, and Huxley, books which reveal the intense personal activity of these giants: and then to turn to Oxford and Cambridge, and realise their sterility at this very time. The younger foundations, such as University College, London, and Owen's College, Manchester, were not yet able to redress the balance: while the Scottish universities devoted their attention almost entirely to the botanical requirements of their medical schools. For more than ten years after the *Origin of Species* had been published, this torpor lasted. As we shall see later, the stimulus towards recovery came not from the universities themselves, but indirectly through the national schools, by means of the Education Act of 1870.

With this unsatisfactory state of the science in Britain we may contrast its position on the continent about the middle of the last century, and particularly that in Germany. The observation of cell-structure as defined by their cell-walls had been vitalised before 1850 by Von Mohl's studies *On the Vegetable Cell.* Protoplasm had been recognised and so designated by him in 1846; as a body bounded by cell-wall in ordinary tissues, and its features were being explored in many laboratories. Schleiden had already decreed that an understanding of the form and structure of plants thus built up of cells must be sought in development: and so a vital morphology was beginning to emerge in place of the poetic mysticism of Goethe. Moreover an era of study of life-histories had dawned. Before the middle of the century Hofmeister had been carrying on his researches upon mosses, ferns, and early seed-plants. From them he acquired the facts from which he revealed in 1851 the life-cycle that is common for all land-vegetation. Thus while official botany at Cambridge had been splitting analytically the varieties of *Rubus,* the laboratory of Hofmeister, first in his shop at Leipzig and later in the universities of Heidelberg and Tübingen, was glowing with a new synthetic flame: and a true comparative morphology had emerged. From 1846 onwards similar studies were being pursued on algae in Germany, but particularly also in France. By a happy collocation of wealth, genius, and artistic skill Thuret, Bornet, and Riocreux summed up their cognate results from the study of seaweeds into their sumptuous *Etudes Phycologiques.* Thus on the continent new views had been revealed on the basis of vital study and comparison of plants lower in the scale, while British botanists for the most part still concentrated their efforts on the systematic treatment of dried flowering plants in herbaria. We now see clearly enough how the British universities had missed the significance of the continental changes of outlook: notwithstanding that their emergence marked the era of preparation of the *Origin of Species.* Even the event of its publication did not immediately break the spell of official sleep in

Cambridge, as I had found in 1875: nor does Scott appear to have fared any better at a like date in Oxford. What then was to be done? Young men who had been made aware of the trend of continental science by such books as that of Sachs felt the years of opportunity slipping away from them. The obvious solution was to pursue the springs of knowledge to their source.

Any serious student should be prepared to travel. It had already become clear to me after having worked through Sachs's text-book that a knowledge of German was essential for the pursuit of botany. His frequent reference to botanical literature plainly indicated this. The first opportunity for learning that language on the spot had come to me in the long vacation of 1875, when Chrystal, a Fellow of Peterhouse and later Professor in Edinburgh, proposed to take a reading party in mathematics and physics to the Austrian Tyrol. The party consisted of six undergraduates of Trinity. W. M. Conway and I travelled out together, visiting various towns and their picture-galleries on the way. After a short walking trip in the Bavarian lakes we found the party gathered up at Sterzing, an old posting town on the southern slope of the Brenner Pass. There we stayed for about eight weeks. We did not read very seriously, but Conway and I did a good deal of minor climbing, including the Wilder Freiger in the Stubai group, as recorded in his *Alps from end to end*. The results of the two months trip were some slight gain in knowledge of physics, a smattering of German, and a practical insight into the overwhelming richness of an alpine flora in July.

The next opportunity offered in 1877, and it was more definitely part of my botanical training. It came after I had attended Vines's lectures in Cambridge, and enrolled myself as his private pupil. He has himself told us how he was not satisfied with such experience as he had gained at South Kensington in 1875-6: it seemed to him essential that he should equip himself for future work by attending some well-known laboratory. His thoughts naturally turned to Sachs at Würzburg, and he had set out in

March 1877. Still an undergraduate, I joined him there in June for the remainder of the summer semester, to learn laboratory methods from Sachs himself. We found the arrangements of his institute to be very simple, though we knew that the greatest advances in plant-physiology had been made there. Vines carried out his experimental work in a room by himself, upon the influence of light on growth. I was often the only student in the general laboratory, where Sachs directed my work in person, and criticised its results. He was a strikingly handsome man with well-cut features, and a keen expression that gained by his backward-brushed hair. It was in the lecture-room that his forcefulness was fully revealed. Some fifty students would assemble there during the 'academisches Viertel', mostly smoking and talkative. But all was still when the professor appeared through a side door, and he began to speak. My imperfect knowledge of German prevented my drawing the full value from his lectures, but clearly they were models of didactic skill. In the laboratory he spoke to me in English. He taught me the Hofmeisterian methods of the time. These were of course pre-microtome days: all our preparations had to be made by hand. For instance, we practised sectioning fresh ovules between finger and thumb: and I remember obtaining really fine hand-sections of the pollen-grains of *Althaea* by embedding in gum-arabic, dried on the end of a cork. Sachs also led me into the practice of exact delineation, being himself a master of it. I remember his saying to me cryptically, pencil in hand, 'every drawing conveys a view': and I have never forgotten his hyperbolic aphorism 'that no one has really seen an object until he has drawn it'. Thus I learned the advantage of hand-drawing, aided or not by the camera lucida. Photography which has so largely superseded it in later years has drawbacks, notwithstanding its greater accuracy. Sachs's own illustrations are among the most explicit ever published, and may be taken as expressing his considered views. Modern photographs often fail adequately to present the object itself, and they never convey a personal view. It

may, however, remain an open question how far this last is a real advantage.

The short course of practical exercises through which Sachs put me in 1877 covered ground already traversed by Hofmeister, Pringsheim, and others. This was exactly what I required at the moment to give precision in preparation, observation, and record. But the real gain was contact with the man himself. The interest of this culminated when at the close of the semester he gave to Vines and to myself copies of his newly published paper, on the arrangement of cells in the youngest parts of plants. For me that gift initiated a long series of developmental studies, ending sixty years later with the last drawing I ever made of a growing point, produced as Fig. 477, in *Primitive Land Plants.*

When we returned to Cambridge in the autumn of 1877, Vines continued his practical class. This was in fact a personal enterprise. The university recognised it officially, and watched the innovation sympathetically: but so far as I am aware it did not directly help. The class, numbering six or eight all working for honours, presented a scene that can never recur. The excitement as we verified facts first revealed in German laboratories was intense. We felt we were seeing, under the personal guidance of Vines, and indirectly of Sachs, things never observed before in Britain. In the cooler light of later days the facts of segmentation which we keenly pursued may not appear so vital as they did then. It was the atmosphere of revelation that mattered, and its effect on those who breathed it.

After taking the Tripos and my degree at Cambridge, the feeling asserted itself that the short visit to Würzburg in 1877 formed an insufficient introduction to German botanical thought and practice. So in the summer of 1879 I again started off, the objective being Strassburg, where de Bary was Professor of Botany; and there again Vines was found, busy in acquiring the technique of fungal culture. In the new German university, established in Strassburg after the Franco-German war, particular care had been

PHOTOGRAPHIC GROUP OF THOSE WHO WORKED IN THE BOTANICAL LABORATORY AT STRASSBURG, UNDER PROFESSOR DE BARY, IN THE WINTER OF 1879–1880

In brackets their academic destinations are given

(12) (11) (13)

(4) (5) (6) (7) (8) (9) (10)

(2) (1) (3)

1. Professor A. de Bary
2. E. Zacharias (Hamburg)
3. ?
4. F. O. Bower (Glasgow)
5. Leo Errera (Brussels)
6. ?
7. G. Klebs (Heidelberg)
8. R. Pirotta (Rome)
9. A. F. W. Schimper (Baltimore)
10. O. Mattirolo (Turin)
11. S. H. Vines (Oxford)
12. The Laboratory Attendant
13. The Head Gardener

taken in the selection of a strong professorial staff. A natural choice had fallen on de Bary for the Chair in Botany, not only on the ground of his outstanding scientific claims, but also of his Alsatian origin. I spent a whole academic year in Strassburg, receiving from the professor much kindness and technical help. Solms-Laubach was there as Privat-Docent, working at Gymnosperms. He had a separate room, and we seldom saw him. Klebs was assistant, constantly present and conversational. But the direction came personally from de Bary, whose private room opened out of the general laboratory, and he was always accessible. The buildings were those of the old French 'Académie', low-roofed and ill-lighted: while the botanic garden was little more than a closed yard, within the old French fortifications then being demolished. The ring of the German earthworks, moats, and isolated forts was far advanced in construction, but the new institute for botany and the botanic garden outside the old line of fortification still existed only as a scheme.

There were in the laboratory at that time few ordinary German students: but it harboured half a dozen nationalities among its regular workers. There was a Belgian, Errera, afterwards professor in Brussels: a Swiss: two Italians, Pirotta later professor in Rome, and Mattirolo, in Turin: an Alsatian, A. F. W. Schimper, afterwards of Baltimore: while Vines and I represented Great Britain. Shortly before our arrival the American, Farlow, had here discovered apogamy in ferns: and Bayley Balfour had investigated *Halophila*. An intermittent stream of visitors passed through, such as Zacharias, Stahl, Victor Mayer, and Büsgen: while Francis Darwin, Elfving, and Marshall Ward came together on a visit from Würzburg, where I believe they had all been working under Sachs. Strassburg was then the vital centre of academic botany in Germany, for de Bary was in 1879 at the full plentitude of his powers. As a lecturer he had none of the artistic grace of Sachs: his effect depended on the substance of his discourse rather than on its delivery. During the winter he gave advanced lectures twice a

week, on anatomy, each of two hours duration. Personally I was firmly held throughout those long sittings: they were based upon his *Comparative Anatomy*, published some three years before.

During that academic year de Bary put me individually through a course of general anatomy following the lines of that great work. Under his guidance I carried out a minor enquiry into the growth in thickness of cell-wall in the sclerotic endodermis of *Dracaena*, in which I practically demonstrated a process of apposition, then a burning question. But the results were never published. In the spring of 1880 I worked on fungi, and particularly on the rusts, which were a speciality of de Bary. Later I completed some observational work on the development of the conceptacles of the *Fucaceae*, the results of which were written up and published after my return. In the later spring of 1880 I came home, and within a few weeks found myself engaged as assistant to Professor Daniel Oliver at University College, London.

Looking back upon this post-graduate period, there is no doubt that it was necessary at the time, in order to become personally acquainted with continental methods. Going to foreign schools was the readiest way of making up that backwardness which had resulted from the academic apathy of a previous generation of botanists engaged in teaching in Britain. A broad outlook upon the circumstances of the time may help to explain how it was that they had failed to keep pace with those advances in laboratory technique and observation which had been growing up on the continent: and perhaps in some degree to excuse their lapse. A great expansion of Imperial interests had taken place in the early Victorian period. The whole energy of Kew, and of the British Museum, of Edinburgh and Glasgow, and in a minor degree of other centres, had been concentrated upon the floristic exploitation of the British Dependencies. India in particular had pressing claims. The cataloguing of the floras of these lands raised far-reaching questions of geographic distribution, which readily worked in with the then novel views on descent. It was indeed the

commanding interest in such matters which had tended to draw attention away from the intensive study in the laboratory. Even the most active professors of the 'sixties and 'seventies of the last century allowed their interests to be directed into restricted channels by the set of the Imperial stream, though they might well have been spread over the whole field of botanical science. This resulted in a lop-sided state, from which a minor revolution was needed for recovery. The immediate remedy for us, as students, was foreign travel: and its result was that in the 'eighties and 'nineties of the last century there was hardly any prominent teacher of the science in Britain who had not visited continental laboratories. Some had found their future lines of specialisation there. For instance, Marshall Ward, the pioneer of Imperial plant-pathology, was a pupil of de Bary in fungology: and Gardiner, one of the finest exponents of minute histology, carried out his first researches on protoplasmic continuity as a pupil of Sachs. But to others it was not so. In particular to Scott, who studied long enough in Würzburg to allow him to graft its academic laurels upon the stock of Oxford. His foreign training gave only the foundation upon which his great career as a palaeo-botanist was built. In 1882, when he returned from Germany, fossil plants were only beginning to emerge from their long sleep.

As the continental leaven began to work among the younger men, there appeared to be some danger of too violent a swing away from the systematic treatment of seed-plants, with a threatening consequence of an inverted lop-sidedness. But this in Britain could only be a minor risk: for Kew, the British Museum, and Edinburgh will continue to serve Imperial needs, and their influence would always tend to right the balance by a call for experts in floristic analysis and record, with systematic grouping of the higher plants as the final result.

CHAPTER V

LONDON 1880–1885

Returning home in the early summer of 1880 employment was found in London, first as assistant to Professor D. Oliver at University College, and later as lecturer under Huxley at the Normal School of Science, at South Kensington. I consulted the latter in 1884 as to entering a candidature for a provincial appointment and he, a thorough-going Londoner, said he would advise any young man who had confidence in himself, and was already at work in London, to remain there. It is the centre of everything scientific: leading personalities are constantly passing through, and all really important advances in science filter into the work of its learned societies. The five years from 1880 onwards proved to me the soundness of this advice, and London was only left in 1885 under some degree of compulsion, as will be explained later.

In the spring of 1880 enquiries had issued from Edinburgh with a view to the engagement of an assistant to the Professor of Botany. It was arranged that I should visit Professor Dickson himself, and so it befell that I made my first entry into Scotland. An incident during those few days was that I was taken as a visitor to a meeting of the Royal Society of Edinburgh, in its old home on the Mound. Lord McLaren was in the chair, Professor Tait acted as Secretary, and a paper was read by Sir William Thomson, afterwards Lord Kelvin. I little thought then that I should preside over that Society forty years later. No definite offer of the assistantship was made by Professor Dickson at the time, and meanwhile a proposal came to hand that I should act as assistant to Professor Daniel Oliver at University College, London. This was accepted,

and for the summer terms in two successive years I organised, and carried through single-handed, the elementary practical courses. In the winters also I gave some more advanced instruction in botany to a few aspirants for honours: one of my pupils there was Sir Sydney Harmer, late Director of the Natural History Museum at South Kensington. From such incidents it became apparent that the leaven of foreign experience was already working in 1880: and that the senior botanists were ready to avail themselves of it in establishing laboratories of their own. For me the question of Edinburgh versus London had been raised, and under influential advice I chose London.

There is nothing calling for special remark in the work of those two years at University College. No lectures were required from me: but the practical classes were left entirely in my hands. It was the cream of the first-year students in medicine who took the class voluntarily, as shown by the success of many of them later in life. One circumstance has, however, some historical interest, in that the course was modelled on the method of Huxley at South Kensington. But there was in 1880 no laboratory guide-book in botany, nor any prescribed course. The material to be used had to be selected and found, and the practical work on it devised from day to day. Some detailed description of treatment and observation was necessary for the guidance of a large class single-handed. To meet this difficulty sheets of instruction were drawn up for the work of each day: multiplied by one of the crude methods of the time, and circulated to the class. As these sheets accumulated it was realised that the text was being brought together for 'A Course of Practical Instruction in Botany'. But here a complication arose from the fact that Thiselton-Dyer had already conducted practical classes in the summer at South Kensington from 1873 onwards, and that a book by him under this title had for some years been announced as 'in preparation'. There were, however, no signs of its advance. I therefore opened the question directly with him, and he generously made me wel-

come to take his place: he even introduced me to Messrs. Macmillan, with whom he had proposed to produce the book. Vines prepared the introductory chapters on reagents and methods, and after laboratory trials extending over several years the first complete edition was produced in 1885, under the joint authorship of 'Bower and Vines'. Later editions appeared under my own name alone. The book, after changes both of text and of authorship, still exists in a condensed form as *Practical Botany for Beginners.* There is no doubt that the original issues played a useful part in the botanical revival of half a century ago, by providing methodical exercises in the study of structure and development, and in promoting accurate observation. But the book was produced intentionally without the support of detailed illustrations; for these are apt to blunt the keenness of search, by substituting the observations of others for the personal experience of finding out for oneself.

Meanwhile developments had been taking place at South Kensington. The Normal School of Science had been instituted some years before, in near proximity to the Natural History Museum, and other exhibitory and educational buildings which sprang up as an aftermath of the Great Exhibition. Huxley himself was the central influence, and round him as Dean of the Normal School had been gathered a senior staff of exceptional quality. Its function was to supply duly qualified teachers for the Government Schools in which, under Mr. Forster's Education Act of 1870, elementary science was to be taught. Out of a medium of occasional lecture-courses and other discontinuous teaching, together with tests by examination under the Science and Art Department, there gradually emerged a coherent and practical scheme of education, centred in the building opened in 1872, and known then as the Normal School of Science. The courses in biology dated from 1870, and they included a practical study of both animals and plants, as represented by 'types' selected to illustrate the chief characteristics of organic life. But there was soon a separation into

distinct courses for animals and for plants. The latter were inaugurated in 1875, under Thiselton-Dyer with Vines assisting him.

In the first instance the botanical courses thus initiated were held as occasional events in the summer, and in some of these the eye-witness himself bore a hand as a junior demonstrator. But in 1881 it was decided that the teaching should be regularised by the appointment of an official lecturer, who would be a member of the permanent staff. I sent in an application for the post in November 1881, and was appointed to begin my duties in May 1882. The students at these regular courses were to be for the most part teachers in training under the Science and Art Department, though other paying students were also admitted. There were also to be 'refresher' courses from time to time, given in the summer for teachers already certificated.

I have good reason to remember my first lecture in May 1882, a critical occasion enough for one so inexperienced as I was. For the test was accentuated by a message: 'The Dean presents his compliments, and wishes to know if you would have any objection to his presence at your lecture to-morrow'. The Dean was Huxley himself. My reply was that I should feel highly honoured by his presence. He met me next morning on the stairs, and spoke most kindly as we entered the lecture room. He took a place in the middle of the front row; stretching out his legs and burying his chin in his waistcoat, he snorted at intervals. When the dread hour was over he remarked to me cryptically, 'You have told me a number of things I never heard before': and continued, 'May I give you a word of advice as a young lecturer. Lecture your audience, do not lecture your blackboard'. Going up to the blackboard he took a piece of chalk and drew: and looking over his right shoulder towards the auditorium he said, 'Cultivate this attitude'. How many lecturers, never having received so kindly a suggestion, daily violate this simple canon of didactics. The Dean was rarely in evidence during my botanical courses: he never again came into the lecture room, and seldom into the laboratory.

He left the adjustment of all details to his junior staff. That same junior staff of the Normal School had been carefully selected. We formed a light-hearted party at the daily lunch, in the museum to which we had free admission. C. V. Boys represented physics, Japp and Newth chemistry, Howes zoology, and Grenville Cole geology: most of us found our way later into the Royal Society. A table was reserved for us in the restaurant and another for the seniors. I well remember how some humorous ebullition at the junior table would find a staid reflex among the seniors.

In 1884 the season of the elementary class in botany was changed from summer to January and February, and after that followed an advanced course till June. In the same year a short course was initiated in July, on plant physiology with illustrative experiments. These had to be selected carefully, so as to give not only visible but also reasonably certain results: such as movements of plants: transit of water: the interchange of gases: and the relation between exposure to light and starch-production. This short course must have been one of the earliest of such demonstrational courses in the country. Thus the simple instruction in botany initiated in 1873 was already beginning to expand in the old Normal School, by steps leading towards those elaborate developments now seen in the Imperial College of Science and Technology, which are its lineal descendents.

KEW

During five years residence in London my relations with the Royal Gardens at Kew were constant and close, though I was never more than a visitor there: never a member of the official staff. Mr. Thiselton-Dyer, who had been appointed Assistant Director of the Garden in 1875, had examined me for the Cambridge Tripos in 1877, and had later enlisted me upon the staff of demonstrators for his occasional courses at South Kensington. In the early summer of 1880 he introduced me to the laboratory in the Royal Gardens, recently built as a gift to the public by Mr.

Jodrell. Accommodation there for research was granted me by the director, Sir Joseph Hooker: and thus a *pied-à-terre* became available for work, with the resources of garden, herbarium, and library at hand for reference, and for supply of material. Professor Burden Sanderson had recently been at work in the laboratory on insectivorous plants, but now it was untenanted. A pleasant room with a north aspect was assigned to me, which gave opportunity for undisturbed though intermittent work. It was so used during five years, whenever the intervals between teaching duties would permit.

In the autumn of 1880 seedlings of that remarkable plant *Welwitschia* appeared above ground in the propagating pits at Kew, and some of them were sacrificed in order to settle certain questions of its morphology. It is a Gymnosperm inhabiting desert shingles of Damara Land, South Africa. Its stunted stem widens out upwards into a rugged woody basin, bearing at its margin two gigantic leaves: these are torn as they grow old into ribbons, which spread out over the shingle. In the adult state it had been magnificently monographed by Sir Joseph Hooker, while its mature structure had been worked out by Professor Daniel Oliver. But the questions were still open how that complex structure could be referred to the ordinary Gymnospermic type, and whether the persistent perennial leaves were the actual cotyledons. From the young seedlings their plumular nature was proved, while much structural and developmental detail was also secured. For me, however, the important point was that in 1880 I was thus brought into close relation with the joint authors of that great monograph.

In the autumn of 1883 Mr. Druery exhibited at the Linnaean Society prothalli borne directly on a plant of *Athyrium*, without the intervention of spores. Material for observation and culture was supplied by him to Kew, and it was handed over to me for detailed examination. The result was a 'Memoir on Apospory,' that is, the direct vegetative transition from the fern plant to the

prothallus. This was published in the *Transactions of the Linnaean Society*. Meanwhile developmental observations were also being carried on, which resulted in 'Memoirs on the Morphology of the Leaf', and on *Phylloglossum*: both published in the *Philosophical Transactions of the Royal Society*. It thus appears that the intervals of teaching allowed of a considerable amount of research being put through at Kew. Presently I was joined there by D. H. Scott, who became later the Honorary Keeper of the Jodrell Laboratory: and by Walter Gardiner. Both were fresh from Sachs's laboratory in Würzburg. The former was at work, as we shall see later, in completing his memoir on the development of milk-vessels, the latter continuing his beautiful demonstrations of protoplasmic continuity through the cell-walls of plants. Thus the Jodrell Laboratory was busy in the years 1883 to 1885. But this period of my official life was abruptly terminated by my appointment to the Regius Chair of Botany in Glasgow, which dated from April 1, 1885, Thereafter I made repeated visits, for spells of work of varying length, particularly in the autumn: but I was never again so constant a worker there as in those fruitful years. They proved the truth of Huxley's advice, as to the advantage of residence in or about London.

The circumstances which led to my giving up that advantage were peculiar, involving others besides myself. The initial step was the appointment of Lawson, then Professor of Botany and Rural Economy in Oxford, to the Directorship of the Government Plantations in Southern India, founded for the supply of quinine from *Cinchona*. The vacancy thus created at Oxford was advertised in November, 1883. Naturally I had thought of it, and I actually put in a candidature. But it was soon known that Isaac Bayley Balfour, who since 1879 had occupied the Regius Chair in Glasgow, was available. He could present credentials which, together with seniority and very varied achievements, made his appointment a certainty. So I settled down again to the duties of my junior but assured position at South Kensington: and to re-

search at Kew in the intervals from official duty. In 1884, however, the prospect appeared of a vacancy in Glasgow, consequent on Balfour's nomination for Oxford. For this I decided not to apply, being satisfied with my position at South Kensington, and with the prospect of its ultimate development. For a whole year, to our surprise, no appointment was made to Glasgow. As a matter of fact, Balfour, though designated for Oxford, had not formally resigned the Regius Chair in Glasgow.

By the turn of the year I had almost forgotten that both of these changes were imminent. Naturally we were ignorant of what was transpiring behind the official screens. On an early day in April 1885, while I was working as usual in the Jodrell laboratory, the door of my room opened, and there appeared Sir Joseph Hooker, Thiselton-Dyer, Professor D. Oliver, and J. G. Baker—the four chief officials of Kew. My initial surprise was increased by what Sir Joseph said. He supposed that I knew about the vacant Chair in Glasgow. I said, Yes, but that I had decided not to apply for it. He then told me that there had been a hitch in the appointment, and that a new candidature was desirable. That he considered it to be my duty to apply, and that if I consented the four of them would support me. I asked for twenty-four hours to think the matter over, though I knew that whatever their reasons I was bound to obey. Sir Joseph said I must tell him my decision next morning. I consulted my family and friends, and duly presented myself at Sir Joseph's office with an affirmative answer. He then told me that there must be no delay: I was to go direct to the Scottish Office in Whitehall, send in my card to the Lord Advocate (there was then no Scottish Secretaryship), who would know all about it. He then gave me four letters—from himself and his colleagues—which I was to hand to the Lord Advocate in person. I did as directed, and after a short interview was dismissed with the remark that I should hear the result in a few days. Very shortly I received a letter from the Home Secretary, Sir William Harcourt, intimating that he had sent in my name to the Queen, but

that the fact must be held as private till it was gazetted. Also that I must put myself at once in relation with the Clerk of Senate of the University of Glasgow, for the duties of the session would begin in about three weeks' time. Thus abruptly my whole programme of life was changed, and a forty years' tenure of office in Glasgow was opened.

The result was that, not by my own choice but by force of unusual circumstances never officially explained to me, I was transferred from a satisfactory appointment in London to the Regius Chair in Glasgow. It was the greatest break of continuity in my life, whether geographically or scientifically. Moreover it happened at a nodal point in the history of British botany. For during the ten years of revival in England our laboratory methods had been fairly levelled up to those of the continent, and the general outlook upon the science widened. In fact the rejuvenation of botanical teaching was now so far advanced that the position of what had been called the 'New Botany' might be held as assured. What remained was the duty of developing it.

CHAPTER VI

GLASGOW UNIVERSITY IN 1885—AND AFTER

To a graduate of Cambridge, who had been a student in two German universities and a member of the teaching staff of the Normal School of Science at South Kensington, the sudden translation to Glasgow University as it was in 1885 brought surprises. The enormous class of over 200 students in Elementary Botany: the smallness of the departmental staff, consisting of the professor and one assistant: the absence of organised advanced teaching: the enforced dependence upon personal initiative, and the defective accommodation and equipment: these were all new experiences and, owing to delay in making the appointment, work under these difficult conditions had to be started with only three weeks' time for organisation, and for putting together an entirely new course of instruction. All these circumstances revealed, what was certainly the fact, that the administration of Glasgow University was then much more mediaeval—amateurish one might even say—than was customary elsewhere.

Its foundation dated from a Bull of Pope Nicholas V, in 1540–1, and its constitution was moulded on the model of Bologna. Mediaevalism had persisted in Glasgow in many forms till the Scottish Universities Bill of 1858. Some relics remain even to this day, such as the mode of election to the office of Lord Rector, for which the students are divided into 'Nations' according to their place of birth. The Act of 1858, under which we worked in 1885, marked a phase intermediate between the foundation of the University and the present time. It committed to a Senate composed

of the principal and professors, the management of the property and revenues of the university previously held by a close corporation of thirteen professors, styled, 'the Faculty'. To the Senate was also committed the regulation of teaching and conferring of degrees. Thus it was virtually the ruling body, and remained so till the further Act of 1889. But the Senate did not enter into details of departmental administration. Not only did each professor personally enroll his students and receive their fees, but he was also responsible for the organisation and expenditure within his department, including payments for apparatus, assistants, and even laboratory attendants. The old regime was in fact a survival from simpler days, and its insufficiency had already become obvious. The introduction of practical teaching in the laboratory was bound to bring it to an end. But in 1885 the financial running of the department was still a personal obligation on the professor.

Against such disadvantages, inherent in the position to which I had been so suddenly appointed, there was to be set the advantage of succeeding Professor Isaac Bayley Balfour. He was a man of ideas, and resolute in carrying them out: moreover he had come to Glasgow in 1879 fresh from continental schools, and with a lifelong acquaintance with the best scientific traditions of Britain. During the five years of his tenure of the chair in Glasgow he had reorganised a torpid department. He had boldly bartered the lecture-room assigned to botany in the plans of 1870 for two smaller rooms that would serve for practical classes, and was content to borrow the use of a lecture-room year by year from some other department. Thus though the accommodation was discontinuous and insufficient, he had left behind him an established tradition of teaching the science along modern lines. I bought from him his students' microscopes and other apparatus, and after three weeks of hectic preparation the session began. Oppressed by the number of the students, far beyond any in my previous experience, I carried through those ten weeks of the summer session of 1885 to their bitter end. It was a desperate hand-to-mouth struggle

against conditions that never would have been possible in a well-organised university.

The personal responsibility of each professor for the whole financial conduct of his department was a survival from primitive times: but it is theoretically quite wrong. That the laboratory expenses should be defrayed from students' fees might be reasonable for elementary classes where the numbers are great and the expenses small: but for advanced classes where the numbers are small and the expenses heavy, and particularly for research, the fees cannot be fixed on such a scale as to cover their much greater cost. The vicious principle was involved that, through the medium of the professor, the relatively poor student and not the rich public would shoulder the cost of that scientific work from which the public would ultimately be the gainer. The reverse should be the rule: but this was not recognised under the primitive conditions prescribed by the Universities of Scotland Act of 1858. These were amended by the Act of 1889, though the change did not come into effect till after 1891. Hence for some six years after my appointment the old unsatisfactory method of departmental finance, stigmatised at the time as 'farming' the chair, was continued.

A fresh surprise came to me in the autumn, when it was realised that no obligation existed in Glasgow University for any teaching of botany in the winter. The professor was therefore free from October till March to go where he pleased. Professor Dickson had wintered on his estate in Peeblesshire, or at the family house in Edinburgh: while Professor Balfour had spent a whole winter collecting upon the Island of Socotra. So I intimated to Principal Caird my intention to visit Ceylon. My health had been indifferent before leaving London, while the summer session in Glasgow had imposed a great strain: so that this holiday in the tropics was of incalculable value. But botanically the time was in some degree wasted, for I had not yet focussed my research on any single branch of botany. My observations and collections in Ceylon were general rather than specific, and much of the material carefully

preserved was never fully worked up. Thus I learned that every travelling botanist should have some definite aim before him: and that he should specially prepare himself by previous study to pursue it. On arrival in Ceylon a bungalow was rented at Peradeniya, close to the celebrated gardens, which were then administered by that sterling botanist, Dr. Trimen, author of the *Flora of Ceylon*. Supported by his experience and advice I collected widely in the garden and in the open. Newera Eliya and the mountain tops of Pedro and Adam's Peak were visited. I waded in the beautiful bay of Beligama, where the sea-floor is overgrown by various species of *Caulerpa*, and many other Algae. Time was also found for examining the antiquities of Anuradhapura, and for a visit to the thrilling Dondra Head, with its little granite temple perched just above high water mark, from which you look southwards over an unbroken expanse of ocean towards the antarctic continent. Thoroughly refreshed I returned to Glasgow in time for the spring examination, and readier to organise the summer session of 1886 than I had been for the hurried scramble of 1885. This was the only winter spent in travel through 40 years. The personal obligation to carry on advanced teaching and research asserted itself, though not as yet imposed as a duty under ordinance. In succeeding winters the time was spent partly in Glasgow, partly in the Jodrell Laboratory at Kew.

Meanwhile I became aware that the Scottish Universities Act of 1889 was in preparation, which was to lay down general principles fundamentally altering their government. It was also to provide for the making of ordinances under which their detailed administration should proceed. This changed entirely the nature and extent of the prescribed duties and obligations of the professor of botany. The chief consequences that emerged under the Act were the transfer of the property of each university to an enlarged and strengthened Court, and the establishment of a central fund into which all the fees of the university were to be swept. On the other hand the salaries of the professors were to be fixed, and no

longer contingent on the number of the students. The members of the junior staff of lecturers and assistants were thereafter to be appointed and paid by the University Court, not by the professors. They thus acquired a new status, becoming a stabilised body which is now represented upon the Senate itself. Lastly, the Court was to assume responsibility for all departmental expenses. These provisions came into effect about 1891. The Universities of Scotland thus passed from an organisation that was mediaeval in character to one more suited to the period in which we live.

A Royal Commission was appointed under the Act, in order to adjust Ordinances regulating details not included in the Bill. Of these none affected departmental organisation so deeply as No. 31, under which a Faculty of Science was instituted in 1893. Provision was then for the first time made for an ordered curriculum in Science, leading to graduation in Honours. It is true that it had been possible at Glasgow before 1893 to achieve Honours in Science, as a branch of the curriculum in Arts: but very few had availed themselves of it: nor were there then any organised courses of advanced study for aspirants in Science. With the passing of the new Act the future prospect cleared: for under Ordinance No. 31 tuition in Science for Honours was to be continued throughout the academic year. This, while it would satisfy the hope of a professor to found a 'school', implied that there would be no repetitions of wintering in the tropics for him, except by special leave of absence. The previous conditions might have condoned the cynical comment of Dr. Johnson, that at his time learning in the Scottish Universities was like bread in a besieged town: every man had a little, but no man had a full meal. The Act of 1889 drew the sting of that jibe, never really justifiable, by providing the mechanism for advanced teaching and individual research. But for botany in Glasgow it was at first rather the shadow than the reality that was provided. For all the practical work of my department was still located in the two small rooms used as laboratories: and it was not till the sixteenth year after my appoint-

ment to the chair that a department worthy of the name was built and occupied.

Glasgow in the 'eighties, being tied to the other Scottish Universities by the successive Acts of Parliament under which they were all administered, was in no worse plight than they, nor indeed than those of England except in point of time. The Scottish universities had been even superior in recent years to the English in their elementary teaching of the science of botany, owing to their better method, and to its close relation to the medical curriculum. This dominated the syllabus of the time, which was mainly classificatory. And so there had been provided a supply of medical men prepared to take up floristic work in India, and in the Dominions and Colonies. It is notorious how large a part Scottish medical graduates have played in the administration of Indian and Colonial gardens and herbaria. This may be traced to the excellent training in systematic botany received in Scotland. But in the Scottish Universities, as in the English, the systematic bias had tended to check the advance in other branches. In the north, however, the stranglehold persisted for some ten years longer than in the south. This helps to explain the relatively backward state of Glasgow into which I plunged in April 1885.

The leeway in respect of accommodation in Glasgow, however long delayed, was amply made up when fresh ground was allotted, and the new departmental building completed at the close of the century. The institutes for engineering and for botany marked the first departure in Glasgow from the scheme of a coherent building for the whole university. Those who planned the quadrangles of 1870 had not forecast the demand for laboratory space that was soon to arise in all subjects that needed to be taught practically. The designs of that period were pre-Huxleyan. In many ways those buildings marked a persistent mediaevalism with its teaching of science based upon the spoken word, rather than on demonstrated fact. For such oral teaching a hide-bound scheme of building may serve well enough. In the plans of 1870

allowance had not been made for that individual demonstration in the laboratory which was so soon to take its place in the various practical sciences. Before long each of these was found to be demanding increased elbow-room and light. Once the break-away is made from the bondage of quadrangles to meet such demands, and the freedom of island sites is established, each institute becomes a unit capable of individual development. The chief obstacle to this later method of academic planning is that it is not economical of ground: but in this respect Glasgow University was so well placed that there was no need unduly to restrict the proposed sites. The number of rooms required for immediate use in the new botanical building was specified in the late 'nineties. The size of the lecture room, elementary laboratory, museum, and herbarium was in each case estimated liberally, with a view to the future. At the time the building was deemed needlessly large: but now, thirty-five years after the event, this pre-vision has fully justified itself. The new institute was opened in 1901 by Sir Joseph Hooker, while Lord Lister and Sir Isaac Bayley Balfour took part in the proceedings. The design was such as to allow of additions being made, particularly for physiological study. For this plans have recently been prepared and carried out, while the present Chancellor of the University has generously defrayed the whole cost of the additional block.

Thus the material aim, sketched out to me by influential supporters for the chair in 1885, has at last been attained. The institute though latterly overcrowded will now give adequate space not only for elementary teaching of large classes, but also for advanced teaching and differentiated research.

CHAPTER VII

DRAMATIS PERSONAE

In the foregoing chapters a short account has been given of the reform introduced into the teaching of science, in the 'seventies of the last century. Having been written by an eye-witness who is a botanist, its scope has naturally been focussed upon his own particular science, though the method on which it was based applies generally to all subjects taught practically. Later chapters will deal with certain botanical results which have followed from that revival. Meanwhile the interest of the story will be enhanced by verbal sketches of those primarily concerned in it, drawn by one who knew them personally. It may be hard for those of the present day to realise the deficiences of that earlier time, so great has been the advance not only in material facilities for teaching and research, but also in the attitude of the student-mind which follows from the careful tuition now available at the early stages of a scientific career. Nevertheless it is well to remember the wise saying of Reinke, that the excellence of the work done is often in the inverse ratio of the facilities.

T. H. Huxley

Among those who brought about that reform of scientific teaching in Britain which has engaged our attention, one figure stands out in respect of the biological sciences as a natural leader: namely, Thomas Henry Huxley. He was born in 1825. After qualifying in medicine he entered the Naval Medical Service, and in 1846 sailed as assistant surgeon in H.M.S. *Rattlesnake*, commanded by Captain Owen Stanley. Her voyage was to be an

exploring expedition to New Guinea, with the object of bringing back a full account of its geography, geology, and natural history. Young Huxley, in addition to his medical duties, was directed to devote himself specially to the last of these. He returned from that voyage in 1850. Through his activity in observation, at sea and on land, he had before his return become a marked figure as a zoologist: and this was confirmed by the publication of his results sent at intervals during those four years to scientific friends at home. In the Huxley Memorial Lecture of 1898 Professor Virchow said that he had by that time become a perfect zoologist, and a keen sighted ethnologist. Practice in dissection under the isolation of a long voyage, and the exercise of a critical judgment had freed him from the formalism of the schools, and had led him to forget their prevalent dogmas: he thus became first a sceptic, and then an independent investigator. This, in addition to his natural qualifications, was the key to that prominent place which he took on his return to London. A Londoner he was and a Londoner he remained, naturally taking there the centre of the stage in questions of scientific education. Officially this worked out in the course of years into his appointment as Dean of the Normal School of Science at South Kensington.

We have already seen how the question of the teaching of science in schools had been raised in 1866, by Dean Farrar at the British Association. Mr. W. E. Forster's Education Act, which provided for such teaching being introduced, was passed in 1870: its application to the National Schools gave Huxley his opportunity. The first step must needs be to provide a staff of qualified teachers. It was at South Kensington that the classes for teachers in training were established; it was here, and not in the universities, that the revolution in the method of teaching the sciences originated. Huxley's first course of instruction for them in biology was held in 1871, and it was a makeshift affair. In the following year the new laboratory was used, the Biological Department occupying the whole top-floor of the block in Exhibition Road

then known as the Normal School of Science. We may imagine what kind of courses in biology were instituted there. Their management was in Huxley's own hands, and he gave the lectures: but he was assisted by Burden Sanderson, Michael Foster, H. N. Martin, Thiselton-Dyer, and Ray Lankester: a most remarkable team. The work arranged for beginners soon took form as the well-known volume on *Elementary Biology*, by Huxley and Martin. But later more detailed courses were devised respectively on animals and on plants separately. The latter were conducted by Thiselton-Dyer, the first of them being held in 1873.

Up to the middle of the nineteenth century authoritative statement by the teacher had been the source of knowledge for the ordinary student in science. It is undoubtedly to Huxley that we owe the initiation of that practical training in the laboratory which has now become general for the biological sciences. His own work had been observational, on the land and in the sea traversed during the voyage of the *Rattlesnake*: it was this method that he handed on to others. He laid special stress upon personal record at first hand, as the leading feature even for elementary students. He did not abolish the lecture-room, but he linked it with the laboratory: so that the student, duly primed by a vivid description of what others had seen, should pass to the laboratory to see, confirm, or criticise for himself. Those who have grown up under this now obvious method will with difficulty realise the change thus brought about. As has been already noted, its effect was at a single stroke to convert each student from a hearer of the spoken word to a potential investigator. On the other hand, the new method would react inevitably on the teacher. Knowing that any or all of his students might form an independent estimate of the matter in hand, he must not only be secure in his facts but also be ready for discussion. Every laboratory class would thus become reciprocally a board of examination for the demonstrating staff. The result was to weld teachers and taught into a compact body of seekers after truth. That was certainly the result in those

pioneer courses at South Kensington, in some of which the eye-witness himself had the good fortune of taking part as a demonstrator.

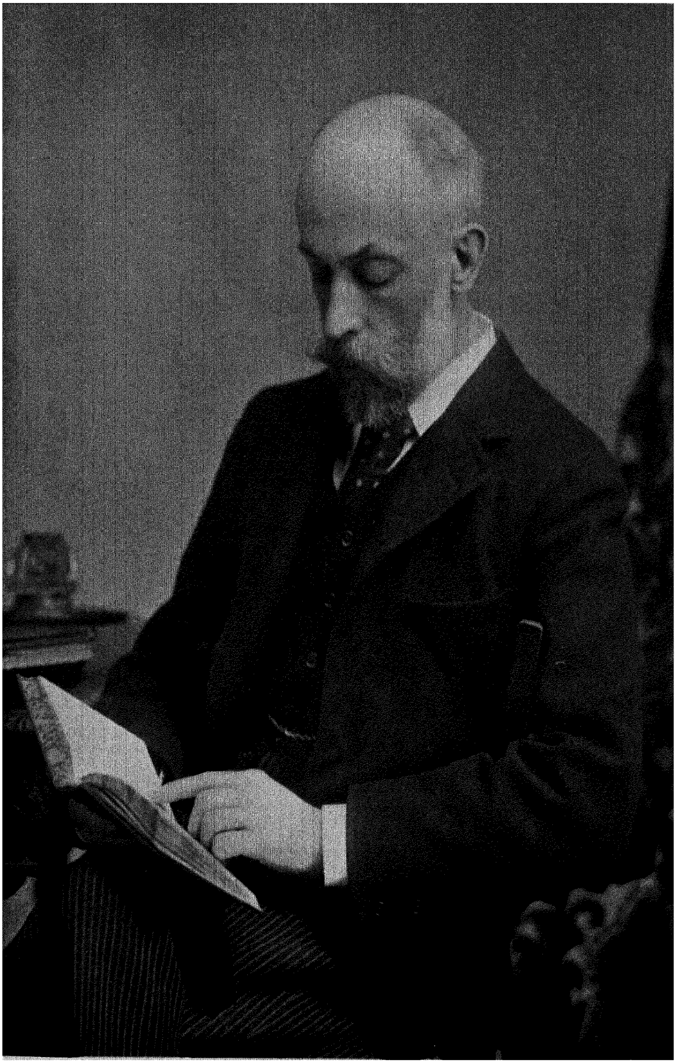
My father had in the 'sixties started a photographic album for contemporary celebrities. Science was represented by Darwin, Tyndall, Huxley, and others, who were attired according to the fashions prevalent when the *Origin of Species* was published. My idea of Huxley had been based on one of these portraits. presenting an aspect as incisive as his speech in debate. Up to 1882 I had never seen him close at hand. On coming into near relation with him officially I found him to be a man of medium height, with well-knit figure, rather greyish complexion, clean-shaven, but with side-whiskers and plentiful grey hair worn rather long, and brushed sharply back from a face that bore an eager and vivid but kindly expression. His well-cut and fashionable clothes could hardly have come from any other source than Savile Row. These together with spats and neat boots, all conveyed the impression of a man of the world rather than a pundit. A year after I was appointed lecturer in botany under Huxley he became President of the Royal Society: there was reason to think that he carried out the duties of that exacting office to the detriment of his health. His attendance at the Normal College became henceforth less regular, and his teaching duties devolved more and more upon his assistants. I left South Kensington in 1885 for Glasgow, and rarely saw him after that date. (See Frontispiece.)

It has been said of Huxley that if he had contributed nothing to science he would have survived as an exponent of the English language. As a philosophical essayist he was peerless. All must be impressed by the clearness of his writing, but more perhaps by the transparent honesty of his thought at a time when clear thinking was of supreme value in guiding the public mind, distracted from its usual grooves by that greatest innovation of modern times, the new outlook on Evolution. He may be described as the Apostle of Agnosticism: suspense of judgment, combined with hope.

W. T. Thiselton-Dyer

While Huxley thus brought new life into the study of biology at large, it was Thiselton-Dyer who, in taking charge of the courses in botany at South Kensington, did the same for this special branch. He more than any other person guided the fortunes of that science during the critical 'seventies of the last century. His direct influence on the progress of botany in Britain fell most effectively within the period 1870 to 1880, and the climax of it certainly lay in the courses of 1875 and 1876 at South Kensington. It may be a revelation to those who have grown up in the traditional methods of teaching botany in later years to learn how much they owe to Thiselton-Dyer, for probably they think of him chiefly as an administrator, and a most efficient Director of the Royal Gardens at Kew. He was born in London in 1843, and having passed through King's College School he graduated from Christ Church, Oxford, with a Second Class in Mathematics and a First Class in Natural Science. In 1868 he was appointed Professor of Natural History in the Royal Agricultural College at Cirencester. While there he collaborated with Professor Johnson in the production of *How Crops Grow*: a book which indicates the physiological trend of his teaching. In 1870 he was transferred to the Chair of Botany in the Royal College of Science in Dublin: but there are no clear records of innovations in his method while in Ireland. As we have seen, Huxley's first course in Elementary Biology at South Kensington was given in 1871: but in 1873 a separate course in botany was devised, which was conducted by Thiselton-Dyer. It seems to have covered only three weeks. The best description of it is given in his own words, as taken from his Preface to the *Course of Practical Instruction in Botany*, by Bower and Vines, published in 1885. It runs as follows:—

"In 1873 I was invited by the Science and Art Department to conduct a course of instruction in what is now the Normal School of Science at South Kensington. It was a condition of the under-



SIR W. T. THISELTON-DYER
From a photograph by Elliott and Fry
Lent by Lady Thiselton-Dyer

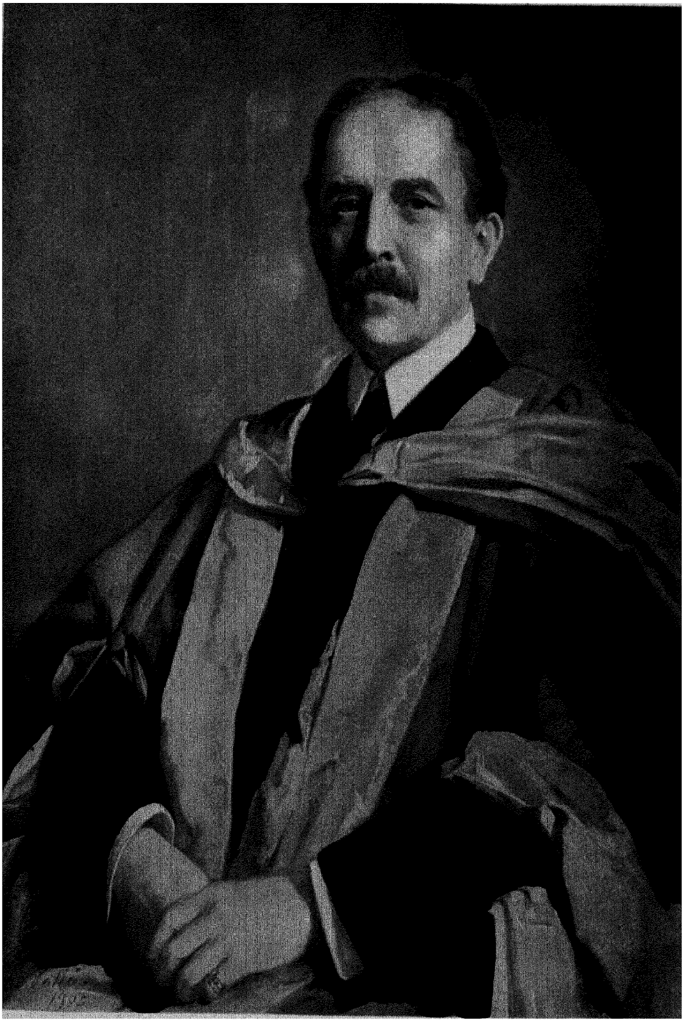
taking that the instruction should be carried on continuously from day to day, and throughout the working hours of each day. My friend Mr. Lawson, late Professor of Botany at Oxford, was so good as to give me his assistance. We had the use of Professor Huxley's convenient and well-appointed laboratory, and we determined to attempt a course of instruction which should embrace the leading morphological facts of every important type in the vegetable kingdom. We, in fact, resolved to adopt exactly the same plan of work as Professor Huxley in his own teaching had found convenient for the animal side of morphology.

"At this time, as far as I am aware, no previous attempt had been made in this country to give an extended course of botanical instruction of this kind. Professor Lawson and myself found our difficulties scarcely less considerable than those of the students. The interest, however, which the novelty of the new method excited in the class soon became very obvious. The enthusiasm of the more skilful students at once stimulated and assisted us, and at the conclusion of the course we found that there was scarcely anything of importance in the rather comprehensive range which had been attempted which the students had not been able to study, examine, and draw for themselves."

It was not only by his organisation of a new style of teaching botany that Thiselton-Dyer played his part in the years from 1870 onwards. He was also at that time when didactic methods were plastic a very active examiner, particularly in the universities of London and Cambridge: and thus he was able to influence the coming generation of teachers not only by classing them, but also by shaping their careers. He examined me in 1877 for the second part of the Cambridge Tripos. The tests would to-day be thought singularly elementary and simple. But at that time the cutting and staining of sections with a view to the detailed recognition and drawing of the tissues was in its infancy in Britain. Vines tells us that in 1874 he, though already a B.Sc. of London with botany included in the schedule, had never seen a microscopical section of

any part of a plant. I was, however, one of the few youngsters lucky enough to have learned Hofmeisterian methods and drawing from Sachs himself. I went into the practical examination in botany for the Tripos at 10 a.m., with lunch in my pocket, expecting a six-hours sitting. I was given a fresh seedling of a kidney bean, and told to make preparations and drawings illustrating the structure of the root. About 1 p.m. Dyer appeared, and asked how the work was going on. I replied that I had done what was required. He then examined me orally on the Characeae. I soon discovered that we both drew our information from the same source, which was Sachs's text-book. Presently he said, 'That will do, thank you'. I asked, 'Is that the whole examination, sir?' He said, 'Yes': and I replied, 'Thank you, sir, and I have spent a very pleasant morning'. Being a really able examiner, he probably found this skeleton examination in the laboratory sufficient for a favourable result.

In 1875 Thiselton-Dyer was appointed Assistant Director of Kew. From that time onwards the call of official duty tended to divert him from teaching, and ultimately from examining also. Nevertheless he was called upon to arrange and carry out at South Kensington the more extended courses of 1875 and 1876 which, as has been noted, may be held to have formed the climax of his teaching period. In later years it was chiefly by suggestion and criticism, by affording opportunities for work to others, and by selection of those who proved themselves fit for appointments, whether academical or imperial, that he continued to be a real power in the movement which he had initiated. The secret of his success was that he was fearless in the advance of proposals fully thought out, and a master of organisation in securing results. But personally he was not a man of robust health, which made more remarkable his effectiveness, not only at the Normal School but also as Director of Kew. His actual achievement as a teacher at this period, with its climax about 1876, must always stand as marking a critical point in the history of botany in Britain:



PROF. S. H. VINES

From the portrait by the Hon. John Collier, 1905

In possession of the Linnæan Society

though his work at South Kensington appears in his life only as a passing incident during his tenure as Assistant Director, and prior to his notable career later as Director of the Royal Gardens at Kew.

SYDNEY HOWARD VINES

In organising the course of instruction in botany at South Kensington in 1875, this time to be spread over eight weeks instead of three as in 1873, Thiselton-Dyer invited Vines, who was still an undergraduate of Christ's College, Cambridge, to assist him. Dyer had made Vines's acquaintance in the previous year, and found in him a willing demonstrator for the more extended course in London. On the other hand, as we shall see, it was Vines who introduced this 'New Botany' into Cambridge, which was the first of the universities to adopt its methods.

He was born in London in 1849, but some years of his boyhood were spent on his father's ranch in Paraguay. He was first put to school in Germany, and later at Surbiton. Under parental guidance he was originally destined for a medical career. This had actually been started by his entry at Guy's Hospital, where he developed a special interest in animal physiology. But soon he was discovered by Thiselton-Dyer, who diverted him into the new botanical stream. The change did not meet with parental approval, a fact which made transition to botany difficult for Vines. But a scholarship at Christ's College, Cambridge, First Class Honours in London, and the same with distinction in botany at Cambridge, appear to have smoothed his way in the end. In 1876 he was elected Fellow and Lecturer in his College. I attended his course in its second term, and continued this, together with private tuition from him till the second part of my Tripos. The lectures were given in the College, where the only illustration available was by drawing on a small blackboard. At first there was no practical class, probably owing to Vines's own sense of unpreparedness which, as we have seen, drove him abroad for further instruction. But in the autumn of 1877, after his return from study

under Sachs at Würzburg, he instituted the first practical class for botany in Cambridge, in a room lent by Professor Michael Foster. The microscopes and other apparatus were supplied at Vines's own cost. Those who witnessed these earliest steps in introducing the 'New Botany' into Cambridge will be best able to estimate the debt owed to Vines as the personal re-founder of its School of Botany. It very soon expanded under the hands of those who were for the most part his own pupils. But in 1888 he was appointed Professor of Botany in Oxford, where for thirty years he devoted himself to his department—the most compact, beautiful, and historically the most venerable in Britain, with its walled garden alongside its laboratories. Following on the reorganisation which had been carried out during Balfour's short tenure of the chair, there was no need for Vines to repeat at Oxford the pressure that had marked his earlier years in Cambridge: and his tenure was one of placid efficiency.

Having been a pupil of Vines, both by attending his lectures and in private, and having visited two German universities in his company, and made holiday excursions with him in the Black Forest and the Vosges, I had found him in these early days a delightful companion. Alternately he would be light-hearted and humorous, or serious and even pensive. A good linguist and a competent musician, I knew him best at this irresponsible period. Later in life, and especially at Oxford, he allowed himself to be so far anchored to his department and his own garden on Headington Hill, as to become almost a stranger in meetings of the London Societies, or of the British Association. The consequence of this was that to the younger generations he became a name rather than a personality, greatly to the loss of botanists at large. But an exception came in the years of his presidency of the Linnaean Society, an office that gave him undisguised pleasure.

Vines's style even as a young lecturer was easy and effective, with occasional flashes of humour. Even in the 'seventies it was apparent that his mind was acquisitive and critical rather than

constructive. He would give a most exact and luminous analysis of the work of others, and balance conflicting opinions to a nicety, giving evidence of a wide study of sources. But in those days he rarely wound up a discussion with any expression of his own definite conclusion. It may be a question for students of didactics how far that method is effective as against an enthusiastic advocacy of a personal view.

Returning from this biographical sketch of Vines himself to the part he took in the early courses at South Kensington, we may gather from his own account of his experiences, embodied here in a condensed form, what was the nature of the instruction given by Thiselton-Dyer in 1875-6. Stimulated no doubt by his work in editing the English edition of Sachs's text-book, which appeared in 1875, Dyer aimed at continuing the reformation begun in 1873. At that time the teaching of botany in Britain had come to mean little more than the study of the flowers of Phanerogams, with only such reference to other parts of the plant as might be necessary for determining the natural order. His idea was to teach not one fragment only of botanical science, but to revive those branches of it which had fallen into neglect. Accordingly he planned the course so as to include Angiosperms, Gymnosperms, Pteridophyta, Bryophyta, and Thallophyta. The study of such a series would give the student an idea of the vegetable kingdom as a whole, including the main features of its classification, comparative anatomy, and physiology; and prepare him for the intelligent study in detail of any one of the groups of plants to which he might wish to devote his attention.

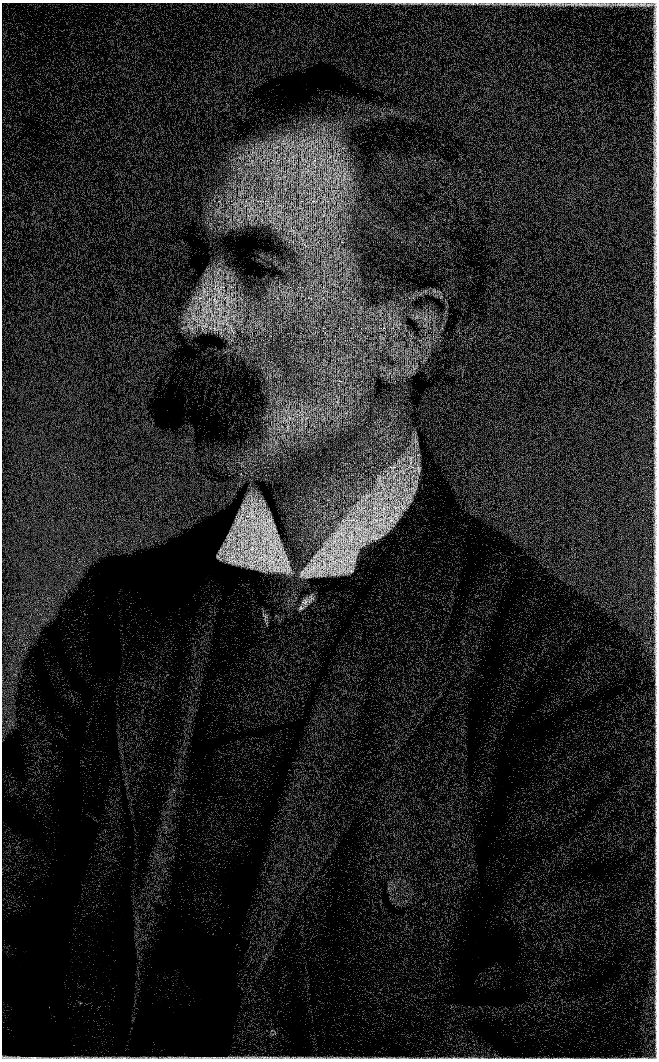
Thiselton-Dyer was a good lecturer though he had not the charm of style that Huxley possessed. His lecturing was, however, less remarkable than his general conduct of the course. As Vines relates, he carried it through with that capability which was characteristic of all his work. It was a revelation to one who for the first time realised that botany was a science worthy of a life's devotion. This course confirmed the start in 1875 of the new move-

ment in England. It succeeded where a similar effort by Henfrey twenty years before had not. It re-established the study of the plant, whether higher or lower in the scale, as a living organism, not merely as an herbarium specimen. That this should have come about is to be attributed in no small degree to the fact that Thiselton-Dyer was himself deeply interested in both the systematic and the biological sides of the science.

No known schedule remains of the work actually carried out in the practical classes of 1875-6. The observation of living specimens of various algae and fungi certainly formed an integral part of the day-to-day work. For this the supply of fresh material must at that time have presented great difficulties. Vines lived during the course with Dyer at Kew, and the Royal Gardens would naturally supply much that was needed. But from later conversation I gathered that the net was widely cast. For instance, I was told that the ponds of Hampstead Heath supplied living material of *Volvox*, while the ornamental waters of Hampton Court yielded specimens of *Hydrodictyon*. Desmids were also supplied from bogs in Ireland. There was a sense of enterprise at the back of it all. When I was enlisted as a demonstrator in one of Thiselton-Dyer's subsequent courses, the nascent spirit of the time still animated the students.

H. Marshall Ward

Among those who took the earlier courses in botany for teachers in training at South Kensington, the greatest discovery was in the person of Marshall Ward. He entered the class of Thiselton-Dyer at the Normal School in 1875. As we have seen, Vines was one of the demonstrators to this class, and he writes that so deep was the impression made by Ward's ability and enthusiasm upon his teachers that they urged him, if possible, to enter on a botanical career. He went into residence at Cambridge in October 1876, with a scholarship at Christ's College and, availing himself of all possible opportunities for study in biological subjects, he secured a First Class in the Natural Science Tripos of 1879.



PROF. H. MARSHALL WARD
From a photograph in the Library of the Royal Society

He at once threw himself into research, and I well remember the impression made by his first paper, published in the *Linnaean Journal* of 1880, on the embryo-sac of *Gymnadenia conopsea*. Rapidity of execution and exactitude of delineation were characteristic thus early of one of the most strenuous workers among British botanists. I had already noted these qualities as peculiarly his own in Cambridge, where he was one of that group who formed the practical class in the autumn of 1877. Under Vines's guidance we were busily verifying the facts revealed to British eyes by the text-book of Sachs.

Later Ward studied in the institutes of Sachs and of de Bary. It was in the latter that the fungi claimed his special attention, a group from which he never broke away. This led to his appointment for two years as Government Cryptogamist for the investigation of the coffee disease, which had half-ruined the planters of Ceylon. His experience in the tropics greatly widened his outlook on the plant-world, while it revealed him as a pioneer in plant-pathology. Returning home in 1882 he took up duty as assistant to Professor Williamson in Manchester. But there soon followed for him a period as Professor of Botany at the Indian Forestry College at Cooper's Hill, for which his experience in the forests of Ceylon had specially fitted him: while it has left a permanent mark on his writings, as shown by his later books on trees and timber. He was a man whose talents pointed him out for early promotion to one of the highest positions. I never felt that he had received the recognition that he deserved till 1895, when he was appointed with general acclaim to the Chair of Botany in Cambridge. Its duties fully engaged him for the rest of his active life.

Before all things Ward was an observer. In this day and night were to him alike, for he would follow the behaviour of a living fungal filament the clock round, with a tenacity which can be traced between the lines of his memoirs. We gather from this that his interest was deeper in the life of the individual than in the progress of the race. Moreover his close observation led to a certain

defect in style: his papers read like laboratory notes, so that in fact their prime merit was sometimes thought to be a blemish. On the other hand, he busied himself constantly with questions nascent at the time, which gave his writings a special interest. But he overworked himself: his health was never robust: the enthusiasm that brought him to the front tended also to shorten his days. His supreme effort was the development of the Botanical School of Cambridge. This involved not only the organisation of an efficient staff, but also the burden of designing, detailed fitting up, and entering into a large and elaborate new building. Marshall Ward saw it completed, and it was opened by His Majesty King Edward VII, in 1904. But Ward himself died in 1906, worn out before his time.

In this chapter four of the chief actors in our botanical drama of the 'seventies, and their several shares in it, have been sketched. But others of the time also took part, though less directly, and without participating in its earliest phases. In later chapters opportunity will be taken to introduce short sketches of some of those who shared in the later stages of its development.

CHAPTER VIII

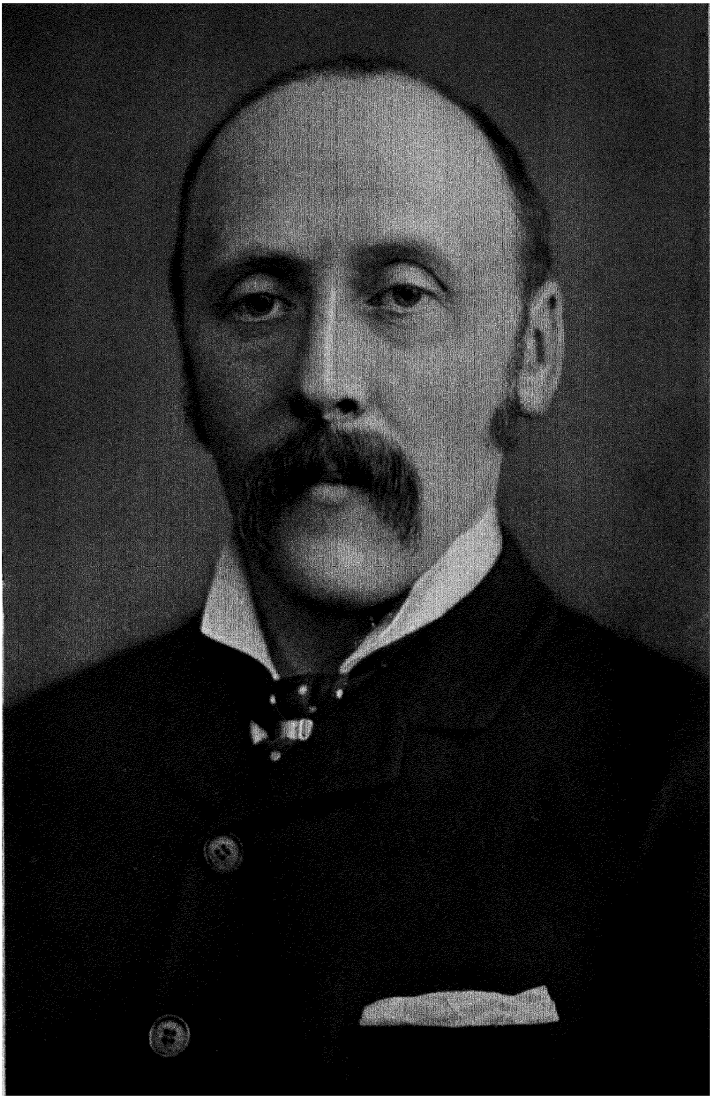
THE OXFORD PRESS AND THE ANNALS OF BOTANY

Isaac Bayley Balfour

We have seen how greatly the Oxford Press, stimulated no doubt by Thiselton-Dyer, himself an Oxford man, had aided the 'New Botany' by the production in 1875 of an English version of the text-book of Sachs. This proved to be only the first contact of the University Press with the botanical renaissance of the 'seventies. Further steps followed as a consequence of the appointment in 1885 of a young and vigorous reformer, in the person of Isaac Bayley Balfour, to the Chair of Botany in Oxford. A short biographical sketch of this remarkable man will help towards a just estimate of the debt owed to him for the part he took at this critical period. Son of Professor Hutton Balfour, the 'old Woody Fibre' of Edinburgh, he was born in 1853 within a stone's-throw of the Royal Botanic Garden. He was singularly favoured in his start in life, drawing his early education from the Academy and University of his native town, and completing his Degree in Science in 1873. But he extended his botanical education by working under Sachs in Würzburg, and under de Bary in Strassburg. Later he acted as assistant to Huxley at Edinburgh in 1875: and as substitute-professor for his father during his illness in the summer of the same year: as dresser to Lister: and as assistant to Wyville Thomson in 1877. Such wide experience gave a catholicity to his scientific interests which never failed him through life. Finally he graduated in medicine in 1877, thus rounding off his varied apprenticeship to science.

As a young man Balfour visited two Oceanic Islands. In 1874 he acted as botanist to the Transit of Venus Expedition to Rodriguez, with results published in the *Transactions of the Royal Society* in 1879. Having been appointed Regius Professor of Botany in Glasgow in that same year, he used the free winter of 1879-80 in making an exact study of the Island of Socotra. He concluded that it had at one time formed part of the African Continent, but had broken away early from Cape Guardafui. His results were embodied in a volume with 300 quarto plates, published by the Royal Society of Edinburgh. However important these expeditions and publications may have been, they formed only a prelude to the real drama of Balfour's life. For he held successively the Chairs of Botany in Glasgow, Oxford, and Edinburgh during critical times of change. Bred in the school of systematic botany in Edinburgh, he was specially qualified to take part in the reforms of teaching described in the foregoing chapters. He was, in fact, the only one of the group of innovators who was fully imbued with, and had himself put in practice the old systematic methods of the herbarium: and so he was able to retain what was best in them, while grafting upon them the new activity in detailed laboratory study. After completely reorganising the botanical establishment in Glasgow during his short tenure there, he was transferred in 1885 to Oxford. He found the ancient garden and small institute adjoining it in great need of attention. He quickly brought the garden, herbarium, and library into a state available for study. In the four short years of his residence in Oxford he found at least one pupil of the first rank, in the person of Sir John Farmer. But the act of Balfour during his Oxford period which has left the most permanent mark upon the science was the establishment of close relations with the University Press, and its enlistment as a foster-mother in the renaissance of British Botany.

This is not the place for completing the story of Balfour's life. It must suffice to tell how in 1888 he returned northwards to fill the Chair in Edinburgh, so long occupied by his father. How he



PROF. I. BAYLEY BALFOUR
From a photograph lent by Lady Bayley Balfour

found the garden, and the teaching department within it, urgently in need of overhaul. How he reconstructed the whole establishment from top to bottom. This became the chief aim of his life. He lived henceforth in and for the garden, and for the university department centred within it. He was not often seen outside its boundary, a subject of remark by those at a distance who neither knew nor understood the work or the man. The real marvel of Balfour's tenure in Edinburgh is that alongside of the reconstruction and administration of the garden, he found time to keep abreast of his science, and to develop the academic side of his duties. For a generation Edinburgh was the chief centre in Britain for the teaching of systematic method as applied to flowering plants. It was a modern organography which Balfour taught, co-ordinated with progressive systematic methods, and with biological knowledge such as few ecologists could aspire to. Finally, a leading characteristic of his official life was his wish to help others from his own ample store of knowledge and experience. Landowners, horticulturalists, foresters and farmers, as well as specialists in pure science, looked to him for advice, and acknowledged its worth. Truly he deserved in the fullest sense his official title as, 'The King's Botanist in Scotland'.

Our immediate interest is, however, his influence with the University Press at Oxford. Following on their production of the English version of Sachs's text-book in 1875, and a second edition in 1882, the Delegates of the Press had also published a translation of the *Comparative Anatomy of de Bary* in 1884, and in 1887 a translation of Sachs's *Lectures on the Physiology of Plants.* In this same year Balfour, still professor in Oxford, entered upon the production of that further series of translations of foreign texts which were needed at the time to complete the revival of the study of botany in English-speaking countries. For that revival had not yet attained the stage of producing its own manuals. In the selection and conduct of that series the University Press found a trusted adviser in Professor Balfour. It was in this capacity that

he ventured to suggest to the Press a further step, of greater importance to the advance of botanical study, which was the foundation of a serial to embody the results of botanical research. But in order to float such a scheme there must be confidence in the quantity and quality of available material. The question would be whether or not a sufficient supply could be depended on. The auguries were held to be favourable: for it was the ambition of those who came to the front, wherever the new methods were adopted, not merely to traverse the ground already known and to verify the results of others, but themselves to penetrate into the unknown. If we turn over the scientific journals of the years before 1875, we find little evidence of this spirit having existed among British botanists. But it at once emerged as a consequence of Huxley's heuristic method.

Already in 1877 Vines had felt this stimulus and, working under the influence of Sachs, he was making observations on growth at Würzburg. It seems natural that these researches should have preceded those of Vines's own pupils. They mark the beginning of that flow of research which called the *Annals of Botany* into being. Meanwhile Bayley Balfour had quite independently been following a like course of production from Edinburgh. In the following year two of Vines's own pupils published records of research. Marshall Ward, the older in years but junior in residence at Cambridge, and myself the witness of these events. Such was the nature of the first fruits of research following on the revival described in the preceding chapters.

Shortly after 1880 there was in fact a rising wave of botanical output, and a probability of its increase as the number of enthusiastic workers grew with the spread of the new movement. Botanists were deeply indebted to the *Quarterly Journal of Microscopical Science* for its hospitality in the early 'eighties: but the great proportion of the memoirs accepted by that Journal were zoological, while Ray Lankester, the editor, was himself a professed zoologist, though with the wide biological outlook charac-

teristic of the Huxleyan period. The result would naturally follow that few botanists would subscribe to the Journal, and the botanical papers would risk being lost in the larger zoological stream. There is plenty of evidence to-day that the botanical memoirs published in the Journal at that date are apt to be quoted by title rather than read. The danger of oblivion grew more acute as the *Quarterly Journal* became more exclusively zoological. On the other hand the rush of botanical production showed signs of swelling into a spate. Balfour grasped the difficulty with both hands, using the good relations he had already established with the Delegates of the Oxford Press to suggest to them the foundation of a separate Journal for Botany. Nothing less would serve to consolidate the position so rapidly developing in the Universities and Colleges of Britain.

FOUNDATION OF THE ANNALS OF BOTANY

A decisive step was taken at a meeting of botanists called by Balfour and Vines in January 1887. Here a proposal for the foundation of a new botanical journal was formulated, for submission to the Delegates of the Oxford Press, with a view to their undertaking its production. The letter to the Secretary containing this proposal is here quoted by permission of the Delegates.

LONDON, *Jany* 20, 1887.

to the Secretary
to the Delegates of the
Oxford University Press.

SIR,

We are instructed by a representative meeting of Botanists held this day, to communicate to you the following information as to a proposed new botanical Journal, with the view of its being laid before the Delegates.

It has come to be generally recognised that there is at present a definite demand for a new botanical Journal, in addition to the existing means of publication of botanical memoirs and general botanical information, and for some months past a movement has been on foot for the purpose of establishing such a Journal. It will be seen, from the list of

the proposed editorial staff given below, that the movement is supported by an important majority of the University teachers of Botany in this country, as well as by teachers in America. Promises of assistance and support have also been received from a number of Botanists whose names do not appear in the subjoined list.

As the result of careful consideration, it has been decided to start the proposed Journal on the following lines:

1. The Journal shall not be published at stated intervals. But whenever, in the opinion of the editors, there is sufficient material, a number shall be issued.

2. The form of the Journal shall be large octavo. Each volume shall average about 480 pages, and about 24 plates.

3. The numbers issued in one year shall, for the present, be not more than sufficient to constitute a volume.

4. It is proposed that subscriptions shall be invited, and that the subscription price of a volume shall be about 21/-.

5. The matter to be published in the Journal shall consist of:

(*a*) Original papers.
(*b*) Reviews and reports on the progress made in the various departments of Botany—also Historical notices.
(*c*) Short notes, and letters on botanical subjects.
(*d*) Index of current literature.

6. The Journal shall be conducted by the following editorial staff:

Editors: Dr. I. B. Balfour, Professor of Botany in the University of Oxford.
Dr. S. H. Vines, Reader in Botany in the University of Cambridge.

With the co-operation in England of:

Mr. F. O. Bower, Professor of Botany in the University of Glasgow.
Mr. F. Darwin, University Lecturer in Botany, Cambridge.
Dr. A. Dickson, Professor of Botany in the University of Edinburgh.
Mr. W. T. Thiselton-Dyer, Director of the Royal Gardens, Kew.
Mr. W. Gardiner, Demonstrator of Botany in the University of Cambridge.
Mr. W. Hillhouse, Professor of Botany, Mason's College, Birmingham.
Mr. George Murray, British Museum.

Dr. D. H. Scott, Assistant Professor, Normal School of Science, South Kensington.

Mr. H. Marshall Ward, Professor of Botany, Cooper's Hill Engineering College.

Professor W. Percival Wright, Professor of Botany, Trinity College, Dublin;

and in America of:

Dr. W. G. Farlow, Professor of Botany, Harvard University.

7. It is proposed that the title shall be 'The Journal of Botanical Science'.

Throughout the proceedings the desire has been expressed that the Journal in question should be intimately associated with the Universities, not only as regards the editing, but also as regards the publication. We therefore address ourselves to you, in the first instance, as the representative of the University Press which has already done so much to promote the interests of Botanical Science in English-speaking countries, in the hope that the Delegates of the Oxford Press may be willing to identify themselves with this further effort in the same direction. We shall be much obliged if you will kindly take the earliest opportunity of bringing this letter to the notice of the Delegates, and of ascertaining from them whether or not they may find it possible to engage in such an undertaking. In case they should take a favourable view of the matter, we should be glad to be informed, as soon as may be convenient, what would be the conditions upon which the Delegates would be prepared to act. Please address your reply to Professor Balfour, who will be very glad to give the Delegates any further information which they may desire.

We are,

SIR,

Yours faithfully,

(signed) ISAAC BAYLEY BALFOUR.
SIDNEY H. VINES.

The names of the botanists contained in this letter, together with the offices which they severally held, sufficiently proved the solidity of the movement. But the Press, as a business concern, would naturally wish some material guarantee for so costly an enterprise as that involved in the annual production of a volume

of about 500 pages, with twenty-four plates. A guarantee fund was asked for, and a sum was raised which ran well into three figures. But after ten years had proved that the enterprise was a success from a business point of view, the guarantee was returned to those who had subscribed to it. As a matter of fact there has never been any doubt of the financial soundness of the *Annals of Botany*. After payment of the cost of production out of annual subscriptions and sales, any profit that remains to the credit of the *Annals* has habitually been used in maintaining its quality, and upholding the volume of the matter to be published. Notwithstanding fluctuations in the supply of articles offered for publication—greatly diminished during the years 1914 to 1919, while the cost of their production was greatly increased during and after the war—the *Annals of Botany* has paid its way for half a century: and there is a sufficient balance in hand as a guarantee against contingencies. Such a result reflects favourably on the foresight of those who guided the enterprise at the outset. For this a deep debt of gratitude is owed to the memory of Bayley Balfour, who was its moving spirit. The foundation of the Annals during his brief tenure of the Oxford Chair was a landmark in the British revival of the preceding years. Not only did it provide a permanent home for memoirs which hitherto were without one of their own, but it also serves to keep its subscribers informed as to the latest movements of British research.

At first the chief editorship seemed to be naturally linked with the Oxford Chair, in the person of Balfour himself. When in 1888 he was transferred to Edinburgh, the reins were taken up by his successor, who had signed with him that important letter to the Delegates of the Oxford Press. For eleven years Vines guided the destinies of the *Annals*, but finally he handed over the duties to Scott in 1899. Thus Vines, as the Professor at Oxford, nursed the Journal through the critical years of its infancy: this formed a fitting complement to his pioneer work in the rejuvenation of botanical study in Britain, and particularly in Cambridge.

CHAPTER IX

FOSSIL BOTANY

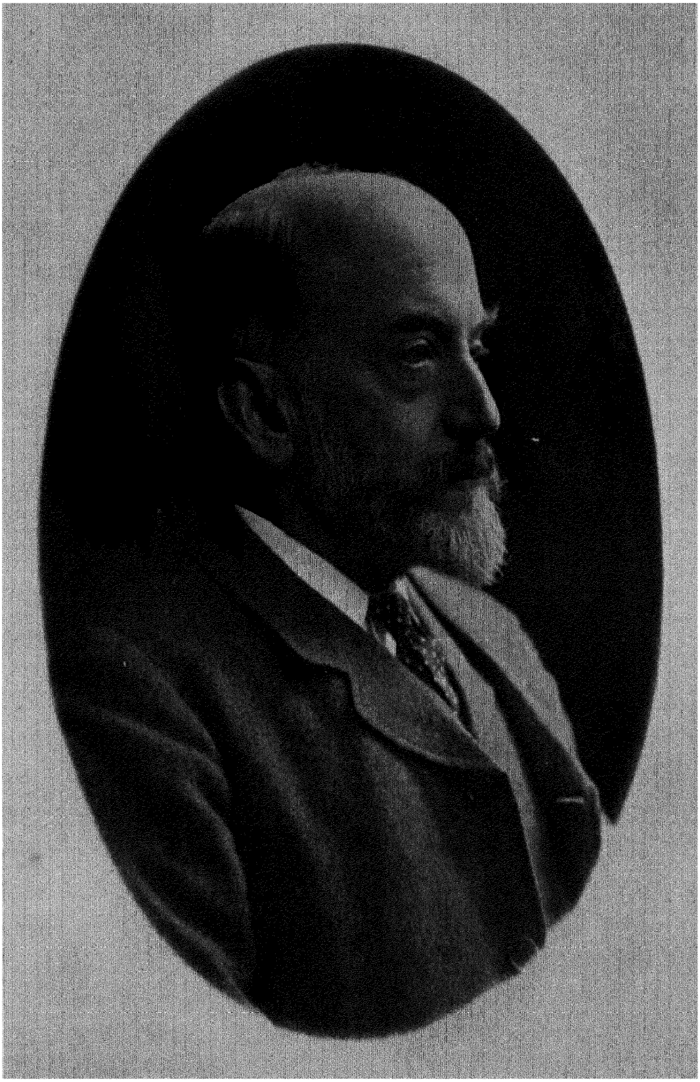
The revival of botanical study in Britain along the lines introduced by Huxley led spontaneously onwards to research. The zeal for personal investigation received further encouragement by the increased facilities for publication of the results offered by the *Annals of Botany*. An ever-widening variety of topics for investigation arose as the science itself progressed, and a tendency to specialisation along any of those diverging lines of enquiry naturally followed. The few memoirs already quoted show how various were the subjects that attracted individual minds, even at those early stages: and in later years their number and divergence has rapidly increased. There is no need to enumerate them here. It may suffice to select for illustration a branch of the science which has become increasingly prominent since the renaissance of the 'seventies. It is that of Fossil Botany, so closely associated with the name of D. H. Scott, himself one of the pioneers of the revival.

Though the branch of palaeobotany dates back to the early part of the nineteenth century, and even further, it could not be pursued with full effect without the aid of the microscope. Observations on fossilised specimens, so often compressed and flattened, and with their tissues imperfectly preserved, need to be interpreted comparatively according to an adequate knowledge of the external form and internal structure of living plants. Hence the microscopic study of fossils would naturally lag behind advances in the morphology and anatomy of their living correlatives. Much pioneer work had, however, been done upon them

before our botanical renaissance in Britain, and some reflection of the results made its way gradually into the text-books. In the edition of 1853 of Lindley's *Vegetable Kingdom*, a representative manual of the period, no mention is made of fossils beyond a curt reference to their existence. But in the excellent 'Class Book' of 1871, by Hutton Balfour, a compact account of the state of fossil botany at that date is included. Not the least important part of it is a very comprehensive citation of the literature on fossil plants up to the time of its publication. But the slightness of the general hold that the study of fossils had obtained at a still later date is indicated by the fact that, in the fourth edition of Sachs's text-book there is hardly any mention of fossils: nevertheless long footnotes were added by Thiselton-Dyer, as the editor of the English version published in 1875. Thus among the botanists of this time in Britain the opportunity for the study of fossils was still limited. Scott tells us, however, that a few special lectures on palaeobotany formed part of Goebel's course in Würzburg in 1881. This must have greatly extended Scott's knowledge of that branch in which he became later so conspicuous a leader.

D. H. Scott

Dukinfield Henry Scott was the youngest son of Sir Gilbert Scott, the celebrated architect. He was born in 1854, and educated at home. As a boy he had collected native plants, and had read English translations from the writings of Alexander Braun, Von Mohl, Naegeli, and Hofmeister: while the *Micrographic Dictionary*, with its citations of German literature, helped to awake his enthusiasm. Thus his taste was defined early. But his entry into Christ Church, Oxford, seems to have slackened its stream. There is no record of his pursuing the science at the botanic garden, near as it was to his own college. In fact, though he already had strong natural leanings towards botany, and a most unusual acquaintance in boyhood with the works of leading continental writers, Scott seems to have found its official presentment at



DR. D. H. SCOTT
From a photograph in the Library of the Linnæan Society

Oxford as uninspiring as I had found it to be at Cambridge in corresponding years.

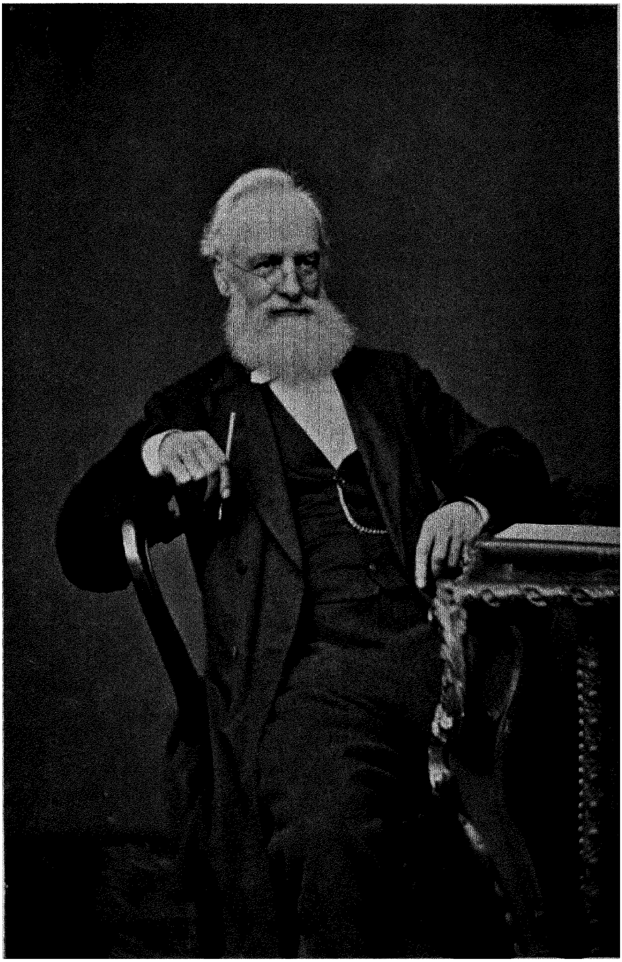
Consequent on his father's death in 1878 he found himself, after graduating at Oxford, possessed of independent means, but without any profession that held him. It was at about this time that I first met Scott, lodging in Cambridge, near to Christ's College, where Vines held a fellowship. Probably this was not accidental but in view of a pending decision. For it was on the advice of Thiselton-Dyer, backed by Vines, that he started out in February 1880 to study botany under Sachs at Würzburg. His Oxford degree passed him automatically into the university as a post-graduate student, and at the end of August 1882 he graduated as Doctor of Philosophy *summa cum laude*. 'The development of the Milk-Vessels in Plants' was the subject of his thesis. Returning to London he succeeded me in 1883 as assistant to Professor Daniel Oliver at University College, and in 1885 as lecturer at the Royal College of Science, under Huxley. During these years, whenever free from duty, we worked together in the same room in the Jodrell Laboratory at Kew: he riding over each morning from Bickley through Richmond Park. We brought our sandwiches with us and shared, with unequally-sized beakers used alternately, sherry that was nursed to fictitious maturity behind a coil of hot-water pipes. Later we were joined there by Walter Gardiner: and the three of us continued our several researches in the same small room, with only two windows. In succeeding years Scott reigned alone in that room, as Honorary Keeper of the Jodrell Laboratory. Such close quarters for research were only possible in those days of hand-sections, and relatively simple methods: but the results in this case were, Scott's later paper on Milk Vessels, some of Gardiner's early papers on Protoplasmic Continuity, and my own Comparative Studies on Apical Meristems of Ferns, and on the Morphology of Leaves.

It was after this period, while Scott was extending his knowledge of the anatomy of living plants and directing the research of

students who came under his teaching at the Royal College, that Professor Williamson took the step which determined his further career. With a view to handing over to one or to both of us the 'good-will' of his own study of fossil plants, Williamson invited Scott and myself to visit him at Manchester. He wished before it was too late to provide continuity in the study he himself had loved. We together paid him, if I remember right, two separate visits during the winter of 1889–90, spending about three weeks in day-by-day demonstrations of the actual specimens upon which the wonderful series of Williamson's published *Memoirs* was based. Our séances lasted throughout the day, in the 'coal-hole', as he styled the small upper chamber where he ground his own sections, and produced laboriously, without the aid of *camera lucida* or photography, those elaborate pencil-drawings which illustrate his works. Scott and I made careful notes, and found that at the close they covered all the more important types of the carboniferous fossils which he had studied. It was an experience that might have determined the course of life-study for us both, as it certainly did for one of us. For Scott himself has said that after this visit to Manchester, he at once became an ardent convert to the intensive study of fossil plants. On the other hand, in order to avoid any possible clash of interests, I undertook still to pursue my cognate but distinct line of research upon living plants, as illuminated by comparison with the correlative fossils.

W. C. Williamson

The story of our host, William Crawford Williamson, is characteristic of his time and well worth the telling. He was born in 1816, son of John Williamson, gardener and naturalist, and finally curator of the Scarborough Museum. As a boy he learned the lapidary's art, in relation to the ornamental 'pebbles' collected from the beach, and worked into brooches and seals. His early education was meagre, though it included natural history and the study of fossils under the guidance of his father. In 1832 he was



PROF. W. C. WILLIAMSON

From a block in possession of the Cambridge University Press

apprenticed, after the manner of the time, to T. Weddell, an apothecary of Scarborough: but in 1838 he was drawn to Manchester as curator of the museum. He entered then the medical school, but in 1839 he passed on to University College, London. In 1841 he took up medical practice in Manchester, and was appointed in the newly founded Owen's College to the Professorship of Natural History, including zoology, geology, anatomy, and physiology! The accessory subjects were successively transferred to other hands, leaving him in 1880 as Professor of Botany, a post which he resigned in 1892.

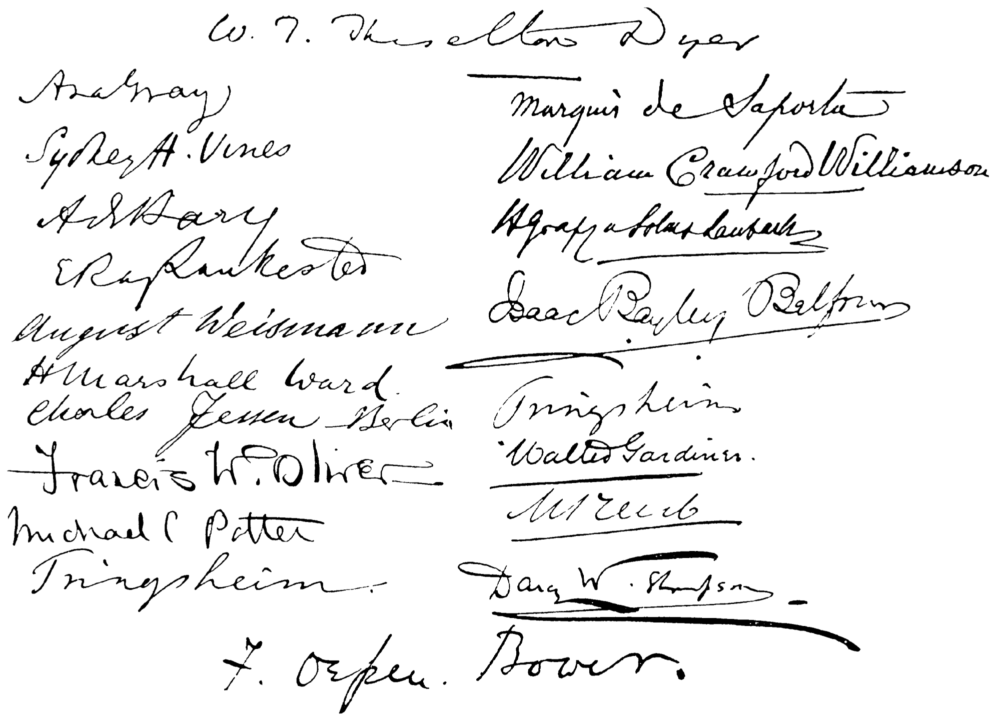
In 1861 Williamson settled into medical practice at Fallowfield, then a village outside Manchester. It was not till 1868 that he returned seriously to the study of fossil plants by means of sections, in the cutting and grinding of which his early experience as a lapidary came to his aid. Shortly after this he began his remarkable series of 'Memoirs on the Fossil Plants of the Coal Measures', published in the *Philosophical Transactions of the Royal Society*, to which he had been elected a Fellow in 1854. The series numbered nineteen, with numerous quarto plates, and they were dated 1871-1893. His work was descriptive rather than constructive. Nevertheless in his Monograph on *Stigmaria*, he drew together all that was known about it up to 1887. Williamson was, in fact, an old-fashioned naturalist, but he takes his place by right as a leader in the founding of modern palaeobotany. His researches were stimulated by his bringing up, which led him to the use of lapidary and microscopic methods of great simplicity, aided by artistic skill and great accuracy. But latterly he felt the need of more detailed knowledge of the anatomy of plants now living to illuminate his results: hence his invocation of help from younger men. Thereby the junction was made between the older and the newer streams of inquiry.

A climax of Williamson's official life came in 1887, when he presided in Manchester over the still undivided Biological Section of the British Association. A special effort had been made to attract

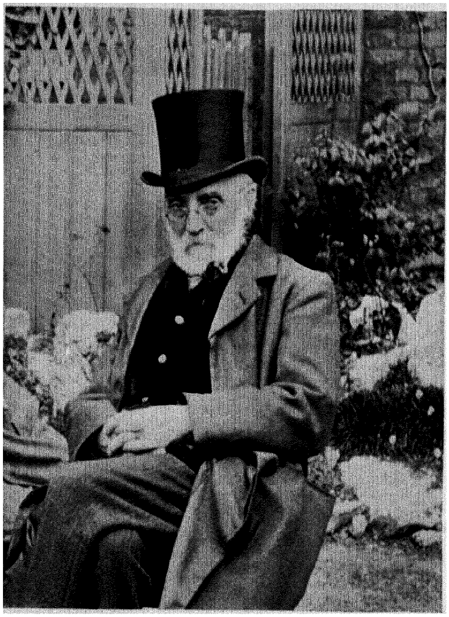
foreigners to the meeting. The group of biologists then gathered round Williamson was so remarkable as to justify the reproduction of the signatures of those present at a dinner given in their honour. The names appear in order as they sat at the table. It includes distinguished zoologists: but the number of notable palaeobotanists present bears special witness to the attraction of Williamson as president.

From this digression we may now return to the discussion of Scott's scientific record. After the visit to Manchester, and, up to the time of Williamson's death in 1895, he and Scott worked together. The results of that happy co-operation appeared as three memoirs published in the *Philosophical Transactions*, their preparation forming for the younger man an invaluable apprenticeship in fossil botany. Scott's individual work as a finished investigator followed. A full bibliography, compiled by his colleague in research, Professor F. W. Oliver, is given in the *Annals of Botany*, vol. xlix, p. 835. None of his published memoirs was more impressive than his masterly elucidation of the structure of the unique and complex cone of *Cheirostrobus*, from the Lower Carboniferous rocks of Petticur: and his recognition of its comparative value as a synthetic type. It may be taken as a fair example of the rest. This is not the place to epitomise the many and fundamental achievements of this great investigator. For such information it must suffice to refer to the three successive editions of his *Studies in Fossil Botany*: a book which is itself a compendium of his own work, illustrating its advance, together with that carried out by contemporary writers. Its style is as clear as the author's own thoughts. For a full generation the influence of D. H. Scott was invaluable as a guide and inspiration to others. His death occurred in 1934.

Scott's achievements in relation to seed-problems may be held as the most notable of them all. The earliest of these were based on the fossil known as *Lepidocarpon*, a club-moss type. But more important still was the recognition of the relation between the fossil



Signatures of those present at a dinner given by members of Section D of the British Association to distinguished Foreigners, at Manchester in 1887



PROF. DANIEL OLIVER
From a block lent by his son
Professor F. W. Oliver

seed known as *Lagenostoma* and the well-known fossil, *Lyginodendron*, now styled *Lyginopteris*: for this led to the establishment of a new class of seed-bearing, fern-like plants, to which the name of Pteridosperms was given. In these innovations he was intimately associated with Professor F. W. Oliver, son of Daniel Oliver, our old chief at University College. Professor Daniel Oliver was keeper of the herbarium at Kew, and a natural leader among the older school of systematists. But he was not exclusively a systematist. He had solid claims also as an anatomist, based upon his early investigations on the Caryophyllaceae, and on *Welwitschia*. He was thus a pre-revival anatomist in Britain, chief among the very few deserving that name. His mantle descended on his son Frank, who succeeded him in the chair at University College. Frank Oliver was a schoolboy when the revival took place. I remember him, on his way daily to University College School, passing the windows of the Jodrell Laboratory at Kew, where in the early 'eighties Scott and I were working. After a distinguished career at Cambridge he proved his quality in general research: but he also was gradually drawn into the stream of palaeobotanical enthusiasm. His detailed descriptions of seeds of the Coal Period led up to his co-operation with D. H. Scott. Together they completed the final observations and arguments that resulted in the foundation of the ancient but newly realised class of seed-plants, called the Pteridosperms. Surely no more generous and genial writing is to be found in botanical literature than Frank Oliver's story of their joint work, quoted in Seward's vivid obituary of Scott in the *Royal Society Proceedings* of 1934. We thus see illustrated in the lives of the Olivers, father and son, the natural steps along lines of anatomical study towards palaeobotany, with one of its greatest achievements as the final result.

R. Kidston

While Williamson and Scott were thus taking the lead in developing a knowledge of fossils in England, chiefly by the

intensive study of internal structure, another palaeobotanist was coming to the front in Scotland: at first by the study of incrustations and hand-specimens, though later by the microscopic examination of their tissues. This was Robert Kidston of Stirling, one of a family well known in the West of Scotland. From his knowledge of the palaeozoic flora he ranked in his later years with Zeiller of Paris, and Nathorst of Stockholm: and he was the last survivor of this illustrious group of friends. As a young man he entered business, but his scientific interests soon asserted themselves, and he gave up banking. About 1880 officers of the Geological Survey in Scotland showed their confidence in his proficiency by submitting to him for determination the fossils collected from the old red sandstone and the carboniferous rocks in Scotland. In 1886 he catalogued the palaeozoic plants in the British Museum, and later he did the same for the Royal Museum in Brussels, and for other continental collections. He intended that this phase of work should culminate in his great enterprise, 'The Fossil Plants of the Carboniferous Rocks of Great Britain': but after four parts of it had been completed, it was left unfinished by reason of his death in 1924.

Apart from this floristic work Kidston co-operated in elaborate anatomical enquiries with two younger men, bred in a later generation of botanical science than himself. About the turn of the century he had become a very particular *persona grata* in the botanical department in Glasgow, where nominally he studied the structure of the living correlatives of the fossils: but actually he brought in exchange a wealth of his own knowledge of fossils, backed by reference to his rich collections in Stirling. In co-operation at Glasgow from 1904 onwards with that very accomplished anatomist, the late Professor D. T. Gwynne-Vaughan, five quarto memoirs were produced on 'The Fossil Osmundaceae'. Again, working this time with Professor W. H. Lang, from 1917 to 1921, there appeared five memoirs on 'The Fossils of the Rhynie Chert', of early Devonian time. The former established on anatomical

DR. R. KIDSTON PROF. D. T. GWYNNE-VAUGHAN

From a photograph taken in Dr. Kidston's study at Stirling

evidence the Royal Ferns (Osmundaceae) as having sprung from a stock that had flourished in the Permian Period, and suggested its relation to the palaeozoic Botryopterids. The latter series of memoirs introduced on detailed evidence of form and structure a new class of plants, the Psilophytales. Their nearest correlatives of the present day are the Psilotaceae, curious 'living fossils' centred in Australasia. The name of the new class was based upon the already known Devonian fossil, *Psilophyton* of Dawson. They were leafless and rootless plants, with distal sporangia: characters that accord with their early horizon. These and other peculiar features point respectively towards an alliance with moss-like plants on the one hand, and on the other with ferns, equiseta, sphenophylls and lycopods. Both of these series of memoirs were very fully illustrated: owing partly to the high skill of Kidston in photographing from fossil preparations, partly to financial help from the Carnegie Trust in their reproduction. They were published in the *Transactions of the Royal Society of Edinburgh*, and take rank among the most remarkable and directly reconstructive contributions to current plant-morphology.

The foregoing paragraphs have been based primarily on the achievements of leading palaeobotanists in this country who have passed away, in co-operation with younger men. But since 1885 active work on fossil botany has been advanced by others, for instance in Manchester, in University College, London, and particularly in Cambridge. Here, from 1898 onwards, its results have taken form in the four volumes of Seward's *Fossil Plants*: while in 1931 his *Plant Life through the Ages* presented a general retrospect of earlier work, there and elsewhere. Such books confirm the emergence of a branch of the science which, in the hands of many workers happily still with us, has become established with a technique and a literature of increasing specialisation. Abroad, with wider fields for collection of material, and with technical and personal resources no less than our own, the study of fossil plants is advancing with a rapidity never before equalled.

The facts derived from fossils carry in virtue of their stratigraphical sequence a positive historical value, which endows them with special weight in argument as to descent. A greater cogency may thus appear to lie with them than with results derived from the living plants brought into comparison. But no morphological conclusion relating to those land-plants that are held as primitive can safely be adopted except after a balanced evaluation of the evidence from both sources. Both of these are inherently deficient. Our knowledge of the palaeozoic flora suffers from the natural imperfection of the record, the frequent rarity or even monotypic character of the specimens examined, and their defective preservation. In the evidence from the living flora the stratigraphical data are insufficient or absent: but here a makeweight is found in the opportunity for developmental study, and a more secure knowledge of anatomical detail, based as a rule on plentiful material: while collateral evidence may be drawn from wide comparison of related genera and species now living. The shortcomings in either case are complementary, and they tend to cancel out when both lines of evidence are combined. But even then the conclusions that emerge cannot, under present conditions, be decisive as to relationship by descent. A balance of probability rather than actual demonstration must for the present be the utmost attainable result.

CHAPTER X

THE EMERGENCE OF PURE PHYSIOLOGY

The new method of teaching introduced into Britain in 1875 at South Kensington was often spoken of as 'Physiological Botany' by the older systematists of the time. In the sense that the objects studied were treated as living things that title was true enough, and the expression had its value as marking a contrast with the study of their dried corpses in herbaria. The fact is, however, that the teaching introduced by Thiselton-Dyer was based essentially on life-histories. It dealt with the phenomena of life, but it was not pure physiology. It was more in the nature of what is now known as Organography, that is, broadly speaking, the description of functionating organisms and the comparative treatment of their parts. The term organography was introduced by Sachs in his lectures in 1882, and it was adopted by Goebel as the title of that great work into which his own far-reaching results were finally condensed. In a general sense it was not then amiss that this new activity of observation and experiment on living plants should have been styled physiological botany, though in 1875 at South Kensington the study of function *per se* was an emergent consequence rather than an end actually defined.

As a matter of history, essays in the theoretical teaching of plant-physiology were already current in this country at a much earlier period than 1875. A very instructive document, preserved in the botanical department of Glasgow University, shows that the discussion of function found a surprisingly large place in the syllabus of a course of general botany, given there by Professor Hamilton towards the end of the eighteenth century. There were three successive Professors Hamilton of 'Anatomy and Botany' in

Glasgow, where their tenures extended collectively from 1742 to 1790. As there is no Christian name attached to the document, nor any date, it is difficult to place it exactly: but internal evidence points to one of the later two as the author: and certainly it dates from a time when oxygen and carbon dioxide were still novelties.

The first part of the course was generally descriptive. Many of the subsequent lectures were devoted to anatomy, based upon the work of Grew and of the phytotomists who followed him. After these came lectures on physiology, in which we find discussed 'the effects that plants produce on the air; the power they have of producing pure air, and of fouling the air; perspiration; secretion; absorption of light; drawing nourishment to the parts of the plant; examination of the fluids of the plant; water not the nourishing fluid, but only the vehicle; properties of sap; motion of the sap in the vessels; these motions explained'. The series ends with a discussion of the question, 'Is there any circulation in plants?' It is probable that lectures similar to these were given elsewhere in Britain towards the end of the eighteenth century.

With such a syllabus as this existing about 100 years earlier, does it not seem strange that the teaching at South Kensington in 1875 should have been regarded as an innovation? What can have happened meanwhile in Glasgow or Edinburgh, in Oxford or Cambridge? Prior to 1875 there is no continuous evidence of the teaching of practical plant-physiology in any of these universities. Even the reputed experiment of Daubeny, in using Magdalen College Port as a colour-screen in the study of photosynthesis, seems to have passed out of practice. Nevertheless I did hear in the early 'seventies, from a pupil of Rutherford, that his biological course at Oxford was run on vital lines. Henslow had attempted physiological courses at Cambridge, while Hutton Balfour had given microscopic demonstrations to his classes in Edinburgh. But these were all discontinuous and isolated efforts. None of them amounted to a formulated course of instruction in the practical physiology of plants prior to 1875. A strange sequel this

in the country of Hales and of Knight. The fact was that Britain had been too busy during early Victorian time in the mechanisation of industry and in railway development, which had made her the workshop of Europe. She was also engaged in consolidating her Empire, with the result that botanically she was counting her assets, not in a rational study of the structure, development or physiology of the plants of her wide-spread Empire: which after all form the basic source of all organic supply: but in cataloguing and describing the plants themselves in the adult but dead state. The botany of the herbarium had stifled the study of structure and function, upon which vitality rests. But a still more potent cause of the deplorable state of the teaching of the science at this time probably was that lethargy, which is apt to invade academic circles when times are prosperous. Whatever the cause may have been, the physiological study of plants was in abeyance, and needed to be rebuilt in England from its foundations.

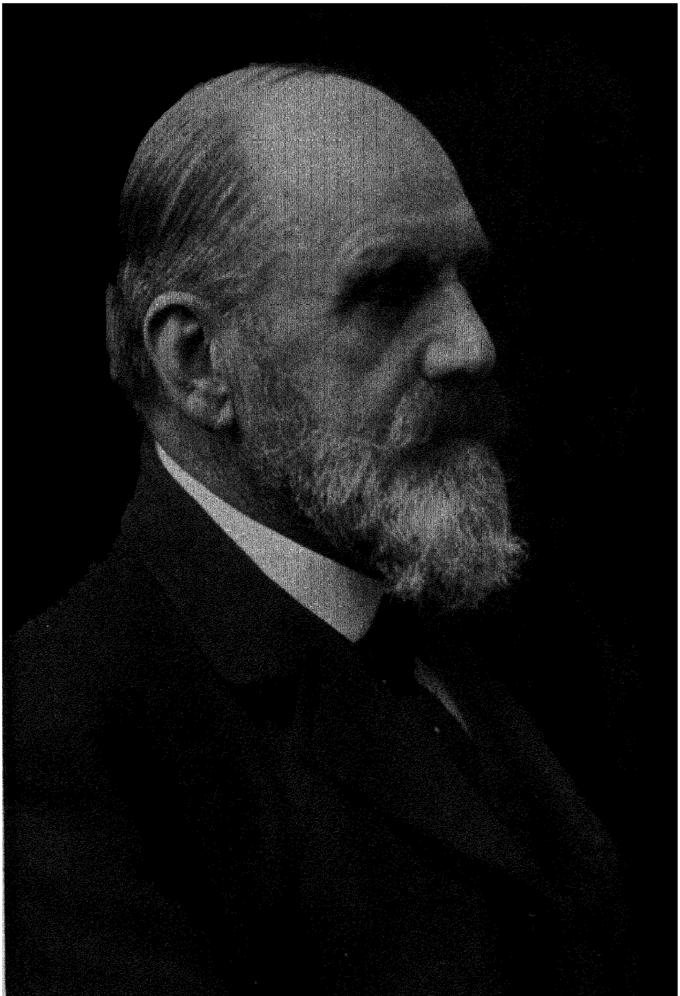
In this the first academic step was taken when Vines, returning home after his visits to foreign laboratories, was appointed reader in botany at Cambridge in 1883. He used his position at once to establish the teaching of plant-physiology there. He was soon joined by Francis Darwin, who was some years his senior in university standing, having graduated in 1870. It might be difficult now to assign to either of these his own particular share in the initiation of that practical teaching of plant-physiology, which has since developed within the botany school in Cambridge. But there can be no doubt of the leading part which Francis Darwin took in its further advance, when he was working single-handed, after 1888, as successor to Vines in the readership. This branch of the science is now established, with varying facilities of space, equipment and staff, in any of the larger botanical departments in the country. But the initiative certainly lay with Cambridge in the early 'eighties. Dr. F. F. Blackman has noted how, 'The year 1881 was marked by a great botanical event. It was then that plant physiology may be said to have crystallised out in its pure

form from the general solution of mixed botanical knowledge. This was the year when the text-book of Pfeffer first appeared. Its publication very shortly preceded the foundation of the readership successively held by Vines and by Darwin, and later by Blackman himself. Thus the world was provided with a comprehensive text, of which the first holders of the readership in Cambridge will not have been slow to avail themselves.

Francis Darwin

Francis Darwin, the third son of Charles Darwin, was born at Down in 1848. At the age of twelve he obtained a good start in a scientific career by entering the Grammar School at Clapham: for Dr. Pritchard's curriculum was exceptional at the time in giving more attention to mathematics and science than could be found in other public schools. Young Darwin passed on to Trinity College, Cambridge, in 1866. There he says that he found the teaching of biology in a state hardly advanced from that which existed in 1828, when his father entered Christ's College. Botanically it was probably not so good: for in 1828 Henslow, as professor of botany, was full of enthusiasm which he conveyed to Charles Darwin on many country walks. It is common knowledge how greatly this professorial friend influenced his career: guiding him towards his decision to accept the appointment to sail as naturalist in the *Beagle*. There was no stimulus like this for his son Francis in the Cambridge of 1866. After graduating with a First Class in the Natural Sciences Tripos of 1870 Francis Darwin went to St. George's Hospital, and graduated M.B.: but he did not practise. Instead of this he returned home to Down, and for eight years he acted as assistant and secretary to his father. His name appears with that of Charles Darwin on the title page of *The Power of Movement in Plants*, published in 1880. On April 19, 1882, his father died: and he was released from this filial duty.

Like others of his period Francis Darwin had also felt the insufficiency of the teaching of botany in Britain; to remedy this he



SIR FRANCIS DARWIN
From a photograph in the Library of the Royal Society

visited foreign laboratories. In 1873 he spent some months in Sachs's institute in Würzburg: and in 1881 he spent a short time in de Bary's laboratory. Such experiences prepared him for aiding in the practical conduct of experiments carried out with his father at Down. Their results are recounted in *The Power of Movement* and other published works. Thus still another of those who actively influenced the revival of botanical teaching in Britain prepared himself to play his part in it. In the early years of the readership of Francis Darwin the course of experimental physiology of plants was greatly developed in the laboratory at Cambridge. Students who there came under his influence will be best able to estimate how much he then did to promote its advance: and how greatly the Cambridge School benefited by his presence. Filial duty had prevented his sharing in the first steps of the revival at South Kensington: but none the less he must be held as one of the leading personalities of the renaissance.

WALTER GARDINER

There was yet another who, though he was too young to share in the actual revival under Huxley, took his part like Francis Darwin in its consolidation at Cambridge in the early 'eighties: namely, Walter Gardiner. During the earliest years of the movement he was at school at Bedford. After holding a scholarship under the Royal Agricultural Society he entered as a scholar at Clare College, Cambridge. A First Class in the Natural Sciences Tripos of 1881 led to his election to a fellowship of his College in 1885, while he also held successively the positions of demonstrator and university lecturer in the botanical department. He was thus an early product of the revival rather than a partner in it. As a master of the finest microscopical technique he was unrivalled, and he found a field for its exercise in examining the minute relations between protoplasm and the surrounding cell-wall. In particular he studied the continuity of protoplasm from cell to cell by fine threads traversing the cell-walls, in the various

tissues of plants. Like others of the time he also had felt the insufficiency of the British laboratory methods, and he followed their example in entering the institute of Sachs at Würzburg. There he found D. H. Scott, already at work. Shortly before his arrival Tangl had demonstrated the fine threads which traverse the cell-walls in the endosperm of *Strychnos*. Sachs recognising that Gardiner was already an expert in microscopical technique, assigned to him the difficult task of widening the detailed knowledge of such structure in the various tissues of plants at large, and particularly in those which respond readily to stimulation. Russow and others were also at work along similar lines. The result was that before long Sachs found himself justified in stating that 'every plant, however highly organised, is fundamentally a protoplasmic body forming a connected whole'. Physiologically the effect of this has been theoretical rather than directly practical. It points to the shoot as a whole being a functional unit, a conclusion which accords with the views of morphologists. The results of Gardiner's researches into minute structure appeared in a long series of memoirs, continued till the turn of the century; the last of them being produced in co-operation with Sir Arthur Hill, now Director of the Royal Gardens at Kew. Gardiner's Fellowship of the Royal Society followed in 1890, and a Royal Medal in 1898. But unhappily failing health has put a term upon the productive period of this brilliant investigator. The names both of Sir Francis Darwin and of Walter Gardiner were among those of the original promotors of the *Annals of Botany*, as may be seen in the original letter to the Delegates of the Oxford Press quoted in Chapter VIII.

Cambridge was thus first in putting into practice that emergence of pure physiological botany noted by Blackman. But it was not long before practical demonstration on the lecture-table presented evidence of the vital activities of plants to students in many departments up and down the country. For instance, in the summer of 1884, a short course on plant physiology had been ini-

tiated at South Kensington, with illustrative experiments, but without any practical work in the laboratory. Lecture-table experiments of a simple nature formed a feature in every summer course given from 1885 in Glasgow. But as the department expanded, and the staff increased with the growing numbers of students for Honours, a lectureship with special reference to plant physiology was founded. A laboratory and greenhouse were soon improvised for the experimental work of the lecturer and his students. But these temporary shifts are now superseded by the building of a new sub-departmental wing devoted to plant-physiology. Glasgow University is here cited merely as an example of the natural trend towards the segregation of physiology as a special branch, following the lines first laid down in Britain in the University of Cambridge.

An incident which threatened to affect the well-being of botany very materially occurred at the meeting of the British Association at Oxford in 1894. In the previous year a separate section of physiology had been instituted, and in 1894 a segregation of the work of the old section D, of biology, was under discussion. It was suggested, as regards zoology, that the physiological side of that science should be drafted to the new section I, while the morphological side should still be retained as the province of section D. To this the zoologists had agreed: what then was to be the course of the botanists? Would they assent to so unnatural a severance? A warm discussion followed, in which zoologists took some part. It fell to me to propose the resolution that the morphological and physiological aspects of the study of botany should be held as forming a natural undivided field for study in the new section K, which was about to be founded. The resolution was carried without serious opposition: and the organisation of work in the botanical section has remained so ever since. But to-day the prevalent custom of joint sectional sittings has softened the edge of that separate allocation of form and function which, in 1894, seemed to threaten the coherence of both of the biological sciences.

Practical application has quickly followed on the advances made in the study of pure physiology in plants. Agriculture, horticulture, forestry, and fruit-farming have all drawn benefit from increasing knowledge, and from the development of method. Plant-breeding has been guided by the progressive study of cytology, while its experimental correlative of hybridisation has already led to marked improvements in cultivated plants, and particularly in cereal grains. The storage and marketing of fruit, and its transport over long world-distances in a gradually ripening state, are being more and more influenced by experiments on gaseous interchange, and the control of temperature: the object being to time the crisis of ripening to the immediate needs of the market. As examples of the post-graduate centres already in existence for special study of these topics, it is sufficient to name such institutions as the Rothamsted Experimental Station at Harpenden: the John Innes Horticultural Institution at Merton: the low-temperature Research Station at Cambridge, and its younger correlative at the Imperial College of Tropical Agriculture at Trinidad. These and others are regularly staffed from the universities and colleges. Thus experiment in the pure physiology of plants is now supplying that scientific basis upon which the application of botanical science to the calls of modern life must needs rest.

Physiological experiment has already been linked with systematic study by the method introduced by Warming, under the title of Ecology. Some danger lurks under the introduction of such a name. It suggests something distinct from other branches of the science, though it connotes a joint aspect of facts and experiments which are essentually those of the systematist and of the physiologist. The virtue of ecological thought lies in the fact that it vitalises the study of the systematist, while it utilises in the comparative treatment of plants that analysis of function practised by the physiologist, which works out in habit and habitat. In point of fact, results drawn from pure physiology are taking their place

in all branches of the science. Even morphology, which seemed at one time to risk its scientific existence by degenerating into mere formalism, has revived through its influence. We have already seen how morphology is now continued under the name of Organography, introduced by the greatest plant physiologist of the nineteenth century, Sachs: and it has justified its name in the twentieth through its latest exponent, Goebel.

CHAPTER XI

THE MORPHOLOGICAL KALEIDOSCOPE

The first English edition of Sachs's text-book, the publication of which in 1875 synchronised very nearly with the revival of botanical study in Britain, included a chapter on the external conformation of plants. A fully organised land plant was then conceived as being built up of parts belonging to certain fundamental categories styled respectively Caulome (or Stem), Phyllome (or Leaf), Rhizome (or Root), and Trichome (or Hair): with certain other accessories. In all such plants these parts co-operated in the constitution of the vegetative system. But there was at that time no separate category assigned to include sporangia, for these were held to be metamorphosed vegetative parts: the equivalents of axes, leaves, pinnae, emergences, or even hairs. They were recognised as such according to their position and mode of origin. This rigidly formal view was, it is true, toned down by Sachs in his text-book, according to his own broader outlook. Nevertheless beneath it still lay the assumption of some ideal plan, which found its most perfect presentment in the higher vegetation of the land. Such a preconceived plan harmonised with that nature philosophy which pervaded botanical literature in the first half of the nineteenth century. Signs of it still remain whenever we speak of 'The Stem' or 'The Leaf'. This plan was based on the writings of Goethe, to whom we owe the word 'Morphology' (1). Plants which did not present this differentiation of parts were designated Thallophytes. For pre-evolutionary botanists they seemed to raise conundrums in its application, rather than guides as to its origin or physiological meaning. Such

difficulties were inherent in a system based upon study of the highest types of vegetation. These early botanists persistently looked through the wrong end of the observational instrument that they used: a practice wholly out of accord with evolutionary views.

The misgivings of Sachs as to the priority of the vegetative system were suggested rather than explicitly stated in his text-book. But they were given full expression in 1881 by Goebel, who had been a pupil of Hofmeister, and assistant to Sachs himself. I well remember how, travelling down one morning to Kew in the autumn of that year, I read in the current number of the *Botanische Zeitung* the aphorism of Goebel, based upon his wide developmental and comparative study of archegoniate plants: 'Sporangien sind so gut Organe *sui generis* als Sprosse, Wurzeln, u.s.w.' (2). Sporangia thus took their place as independent organs, not referable by metamorphosis to any other category of parts. I went straight to Thiselton-Dyer's office in the Royal Gardens to tell him of this new and stimulating view. For me the morphological kaleidoscope had received a jolt, and at once it yielded a new pattern. The focus of enquiry had been shifted from the vegetative organs of the higher plants to the propagative organs of those more rudimentary, such as mosses and ferns: and a stable basis had thus appeared for opinions which were to prove themselves more in accord with evolutionary theory.

A further step in advance consisted in fixing attention upon the carpospores themselves, contained within the sporangium: their production which follows on tetrad-division being a constant event in every normal Hofmeisterian cycle. In like manner the gametes, contained respectively in the antheridia and archegonia, came into greater prominence. Their fusion in fertilisation supplied an alternating event to that of spore-formation in the cycle, while somatic phases or 'generations' intervene respectively as incidents that fall between them. The normal succession of phenomena, vegetative and propagative, could then be graphically re-

presented so as to bring into due prominence the relatively stable events of spore and zygote, as against the more variable somatic phases (3).

All this followed naturally from Goebel's thesis, which established the spore-producing organs, and by inference the gametangia also, as parts of independent order. But the relations of gamete and spore were shortly to be revealed in a new light, reflecting back towards evolutionary origins more directly than any reference to vegetative parts could do. Under the hands of Strasburger and others cytological observation had been advancing in the 'eighties of the last century. Those who had been following the progress of enquiry into nuclear detail, as disclosed by a rapidly improving technique, were prepared for some illuminating generalisation. To them Strasburger's statement in 1894 on periodic reduction came as a natural sequel: thenceforward normal alternation has been bound up with a chromosome-cycle. At the Meeting of the British Association, in that year at Oxford, Strasburger dropped the key-stone into an arch already outlined (4). This event again jogged the morphological kaleidoscope, and again a new pattern appeared, this time in terms of nuclear detail. The Hofmeisterian cycle was thus based upon syngamy and reduction, which marked the limits between the haploid gametophyte and a diploid sporophyte. The stress was now laid upon cytological events, rather than upon the somatic interludes between them.

It is true that many years earlier than 1894 irregularities in the cycle had been recorded: first, when apogamous buds were seen to appear without syngamy upon an apparently normal prothallus (5): secondly, when gametophytic growths were found to be borne on the sporophyte in certain mosses and ferns, without the intervention of spores (6). At first sight such happenings seemed to be altogether subversive of regularised alternation. But so long as the eye rests upon the normal cycle, and particularly on those very constant features in it known as syngamy and reduction, the

Scylla of apogamy and the *Charybdis* of apospory may safely be passed, as *ex post facto* irregularities in the developmental history of a land flora. On the other hand, those events which are recognised as normal accord with the amphibial conditions to which primitive dwellers on land would of necessity have been exposed, if they owed their origin to some aquatic source.

As early as 1882 Weismann had asserted that the birthplace of all animal and plant life lay in the sea (7): and it was generally accepted that the source of both kingdoms was aquatic. The origin of a land flora must needs have involved a transitional stage of *amphibial life*. Further, in 1884 Naegeli pointed out as a fundamental law of organic development, that the phenomenon of reproduction of one stage becomes in the next higher stage that of vegetation: thus introducing the factor of *somatic advance through sterilisation* (8). These two theses supplied the foundation for a biological theory of alternation as seen in the plants of the land. It was indeed high time that this fundamental problem of form as seen in them should be treated from the point of view of general biology, rather than of formal morphology. The leading facts are, on the one hand, that in such primitive types as the mosses and ferns the gametes are motile only in water, while the spores are dispersed in air: also that there is evidence of sterilisation of potentially propagative cells, this supplying an essential step towards vegetative advance. Hence the view was propounded in 1890 (9), more fully elaborated in 1894 (10), and restated forty years later, that alternation as seen in the Archegoniatae has been stabilised in relation to amphibial life (11). But it has only been recognised in late years that this archegoniate alternation should be discussed on its own merits, as not necessarily comforming with that seen in algae and fungi, which are not subject to amphibial stabilisation. In point of fact the doctrine of alternation established by Hofmeister, from the Bryophyta upwards, cannot be extended to all plants (12). The introduction of this biological aspect of the Hofmeisterian cycle again shook the morphological kaleidoscope, and

yet another pattern made its appearance, involving now function rather than mere form, or structure.

In discussing the origin of alternation in land plants it has always seemed natural to seek the source of a green terrestrial flora among the green algae, rather than the red or the brown: and later investigations have not altered this view. Such genera as *Ulothrix*, *Coleochaete*, *Œdogonium*, or *Chara*, have habitually been quoted for comparison. But these all stand cytologically on the haplobiontic plane, reduction being involved in the first nuclear division of the zygote. In these Algae there is no somatic alternation, for none of these have hit off the innovation of postponing reduction so as to interpolate a diploid soma. Here lies the morphological gap between green aquatic and green amphibian life. But if any race of the former had initiated a massive diploid phase, what a future would have opened before it! Nothing less than the capture of the land, provided that the nascent diploid phase could endure sub-aerial life, and were protected against the risks of youth, as the archegoniate embryo is. By this one stroke three biological ends of supreme importance to any land-living organism would have been achieved: (i) multiplication of the possible combinations of hereditary character (13), (ii) the means of a wide spread on dry land by numerous air-borne spores: and (iii) relief from dependence on repeated syngamy for numerical increase, since on land the necessary external water is not always available. Any organism possessing these biological advantages, of which the last is probably not the least important, would leave its haplobiontic ancestors hopelessly behind. This seems to explain the absence of any higher isokont green Algae at the present day, for as Fritsch has suggested, they would have become land-plants (14). Probably the advance on land that was assured by such biological advantages was rapid. The less efficient intermediate types would not survive: hence the Archegoniatae now appear as though they had arisen, like Minerva from the head of Jove, fully armed with an efficient archegonium, and a protected embryology.

In the absence of comparative evidence showing its origin, the archegonium may be taken as a new adaptive organ, giving protection to the germ within. Encapsulation of an embryo in one form or another appears to be essential for the higher life on land, whether animal or vegetable. It has probably originated along a plurality of evolutionary lines. But the chief interest for us lies in the germ, which the presence of the archegonium has made possible on land. The first step in the development of any embryo plant of fixed habit is the definition of *polarity*, that is the distinction of apex and base. These poles are related to the first nuclear figure in the fertilised egg, and their opposing position is confirmed by the first wall of cleavage, which demarcates an epibasal from a hypobasal hemisphere. A general comparison of the embryos of archegoniate plants has shown that, however various the form of the germ may become, the apex is located at, or very near to, the centre of surface of the epibasal hemisphere, and the base similarly in the hypobasal. This is particularly clear where a suspensor is present. The poles define what has been described as *The Primitive Spindle*. This is a general feature in the embryos of land-living plants, though often it is disguised by circumstances of orientation or nutrition that accompany the nursing period (15). Upon this spindle further developments are based. It may preserve and elaborate the simple form, as in the liverworts and mosses, and possibly some of the most primitive of vascular plants: or its form may be complicated by distal branching, or by the addition of appendages, as in the higher vascular plants. But however elaborate the individual may become the germ, with its continued embryology, is as a whole the unit of construction: it is from the first the primordial shoot of Sachs (16).

The simple spindle-form is retained in the Bryophytes as the sporogonium, which shows little or no elaboration of external outline. But as seen in the living mosses and liverworts it may be held to present in varying degree a climax of internal elaboration of the primitive spindle. The result is that a sterile stalk bears up-

wards a fertile, spore-bearing capsule. The apposite term *Telome* introduced by Zimmermann as connoting the structural unit in the vegetation of the land, has been defined as a distal sporangium borne on a stalk (17). The primitive spindle of the embryologist, as seen in the Bryophytes, may accordingly be held as itself representing a primary telome. In the mosses and liverworts the scheme is limited both in size and duration, the sporogonium being dependent for part or the whole of its nutrition, and also for mechanical support, on the gametophyte that bears it. Thus checked by its simple plan and by its means of nutrition, it is doomed to the dwarfed existence which we see.

The converse appears in the unlimited plan of the sporophyte in vascular plants. We have seen how recent years have been marked by stirring events in fossil botany, which throw new light on their morphology, and narrow the gap between the habit of the Bryophytes and of the simplest Pteridophytes. A clearer vision of advancing structure is thus presented in our kaleidoscope. The detailed description of such generic types as *Sporogonites*, *Cooksonia*, *Zosterophyllum*, *Hornea*, *Rhynia*, *Psilophyton*, *Asteroxylon*, and *Protopteridium* (18), with many others, suggest, to those who have followed the story, a prevalent type of early organisation of the sporophyte. It was fixed in the soil independently of the gametophyte: it possessed continued apical growth and branching fundamentally of dichotomous type. In the simplest of them the sporangia were distal, while the cylindrical stalk showed photosynthetic activity with internal ventilation, and there was an internal conducting system traversing the whole. A collective study of them leads onwards to that organisation that underlies more advanced types of land vegetation. Their underground absorptive region suggests a prototype of roots: the dichotomous and even dichopodial branching of the fertile shoots suggests a cladode origin of sporophylls as in the ferns: also smaller outgrowths laterally on some of them suggest sterile enation-leaves, or microphylls, as in the Lycopods. The originally distal sporangia,

often numerous and sometimes themselves branched, may remain isolated as in the Lycopods: or they may be grouped more or less closely together, as in the sori of ferns or the sporangiophores of the Articulatae. Though more highly organised than the sporogonia of the Bryophytes, these Psilophytales are still rudimentary, the type being essentially Palaeozoic, leafless and rootless. Nevertheless they offer unmistakable analogies with the living plants of the land. There is no need here to pursue formal comparison further into detail of actual affinity. These facts and comparisons are sufficiently presented in the general treatise on *Primitive Land Plants*, published in 1935. The attempt was there made to show how, on a wide basis of fact, branched telome-systems may be seen to have been built up from the primitive spindle, or simple telome (17). The most consecutive sequence is seen in the Filicales. It may be traced from such a source as *Protopteridium*, of Devonian Age (18), onwards through the Coenopterids to the ferns of the present day, expanding in elaboration of form and structure. The success of this type in modern times may be measured by the number of living genera and species of ferns. Comparison within the phylum of the Filicales provides the most complete and consecutive lesson in progressive organisation, from the Palaeozoic to the present day, that the vegetable kingdom affords.

From comparisons of such a nature as this a sound foundation may be laid for a general morphology of the vegetation of the land. But its full achievement is not possible as yet. At the moment, a century after Schleiden had tried to convert botany into an inductive science, some eighty years after Hofmeister had published his great synthesis, seventy years after the publication of the *Origin of Species*, and sixty years after the revival of the study of botany in Britain, we stand still facing critical points in the study of form in plants. For there are wide gaps in their organisation, and specially that between the Archegoniatae and ordinary seed plants, which it is difficult to fill by detailed comparison leading to any phyletic conclusion. Instead of attempting this at present it will

be more fruitful to sum up the case for the Archegoniatae as it stands now, in this year of 1937. We need not rest on the defeatist statement sometimes made, that the Pteridophytes, and in particular the Filicales, represent blind branches of descent that lead no further in evolution than what they are. It would be wiser to learn from them such general lessons as they yield in progressive organisation: for the Archegoniatae provide demonstrations of advance that have been successfully put in practice by Nature herself.

We have good reason to adopt the following conclusions, as based on study of the Archegoniatae:—

I. That amphibial life has stabilised the events of the Hofmeisterian cycle, as against the less stable cycles seen in Algae.

II. That the vegetation of the land is chiefly sporophytic.

III. That the primitive sporophyte, as represented by the Archegoniate embryo, shows polarity as the first step in its organisation.

IV. That in the Bryophyte embryo the primitive spindle so defined habitually differentiates a basal and sterile, from a distal and fertile region: thus advancing in its development to the state recognised as a 'telome', but without branching.

V. That organisms of like construction, but dichotomously branched, are a marked feature in the earliest known floras of the land.

VI. That this type of branching is often developed as a dichopodium, and it is liable to pass over to the monopodial, particularly in ferns: thus leading towards a distinction of axis and leaf, the latter being of the cladode type, or megaphyll.

VII. That in certain archegoniate types smaller leaves originate laterally, by enation from surfaces previously unoccupied. These enation-leaves or microphylls may or may not be photosynthetic.

VIII. Thus two types of leaf at least may be recognised, differing in their origin: the one originally fertile is produced by distal dichotomy, the other originally sterile is produced by superficial

outgrowth. But they tend, as their evolution progresses, to resemble one another in form, structure, and function.

IX. That in some Archegoniatae, particularly in the more recent ferns, the leaf-segments may become webbed laterally, to form a continuous expanse, and their veins to become fused, and even reticulate in those of Mesozoic Age. This is a step in method towards a broad-leaved type of vegetation.

X. Where telomes branch a single sporangium may be borne at the end of each twig. In complicated cases they may be grouped in trusses, as in the sporangiophores of the Articulatae, or in the sori of ferns.

XI. Certain potentially fertile cells or tracts of cells within a sporogonium or sporangium may develop sterile, thus adding to the tissue of the diploid soma, through deferring the events of reduction and of spore-formation.

XII. Individual sporangia as a whole may also be aborted; or many, or indeed all of those upon a leaf or shoot. This tends materially to extend the vegetative system.

XIII. Certain leafless branches derived from the spindle enter the soil and ramify there, forming an absorptive system. Though these do not show all the characters of roots, they may be held as prototypes of them.

XIV. All of these changes may be carried out as modifications of primary development. But cambial growth with secondary development is indicated in most of the archegoniate phyla, though in many of them it is not carried into full effect.

Proceeding along lines of analysis which have disclosed such results as these, it has been found possible to apply Zimmermann's theory of the Telome, as a structural unit, very generally to the sporophyte of the Archegoniatae. In particular, the early fossils have greatly aided the view of a transition from the simple telome as it is seen in the Bryophytes, to the complicated amplifications present in other archegoniate plants. The consecutive sequence of the ferns, with their elaborate structure of leaf and shoot, at first

sight seemed to present special difficulties. But close comparative study of the architecture of leaf and stem has brought into sight progressive transformations based upon dichotomy, which result in their outlines as we see them now. Moreover in position and in structure their sori prove to be the result of specialisation of telome-trusses. If this be so for that most consecutive of all evolutionary lines of land plants, it should encourage enquiry by similar methods, in dealing with the flowering plants also: though in these the factors of advance may have been still more varied than those so far recognised (19).

The results thus summarised show how the morphological kaleidoscope has completely reversed its pattern within the last sixty years. So far from any spore-producing member originating as a metamorphosed stem, leaf, or pinna, or even hair, the reverse is seen to be the outcome of comparison, if we proceed in our review from the simpler and more primitive to the more complex and advanced. Moreover, if we take account of the biological conditions of amphibial life, this consolidates the argument from formal comparison. Three successive phases, related severally to external conditions, may be seen in the evolution of the vegetable kingdom:—

I. The *aquatic* which was the most ancient, though it is continued to the present day where the conditions permit. In those which show sexuality the relation of the somatic development to the events of the nuclear cycle is not stabilised except in some instances.

II. The *amphibial*, in which the conditions of life on land have tended to stabilise evolution based on the Hofmeisterian cycle, and to promote the diploid phase. This stage originated in palaeozoic time, and its results persist in the Archegoniatae, with fertilisation by sperms motile in water.

III. The *fully sub-aerial* which is still based on the Hofmeisterian cycle, but with the gametophyte reduced to a vestigial state in relation to the seed-habit: and fertilisation is by a pollen-tube, being thus independent of external water.

The break between the amphibial and the fully sub-aerial type of vegetation is strongly marked. Speaking generally, it involves the advent of broad-leaved foliage. But notwithstanding this and other well-known features, the method of dealing further with flowering plants should be the same as that which has been used with success for the Archegoniatae during the last sixty years. However difficult in achievement, there should be the same upward outlook in comparison, with precedence given to the propagative over the vegetative organs. Help will doubtless come through the further discovery and elucidation of early angiospermic fossils, of which there are already hopeful indications. Lastly, there should be readiness to perceive new factors that have influenced form and structure. On the other hand, we should be prepared to find that certain features that have proved useful in comparison among the Archegoniatae may become subsidiary, or even useless, when applied to flowering plants. Such changes accentuate the break of phyletic continuity which marked the opening of the Mesozoic Period. In seeking to bridge it, the aim of the future should not be merely to trace phyletic sequences, but to visualise the methods of evolutionary advance. We should endeavour first to find out, by close comparison, how the higher plants came to be such as they now are. Phyletic results may follow later.

The relatively new field of experimental morphology will also take its place in working towards a final solution. But its results should be used with as much caution as is needed in the study of abnormalities which, as pointed out long ago by Cassini, may lead to any view that it is wished to establish. Who is to know whether the reactions of to-day would be the same as those of early geological periods? Who can say that the external conditions of an experiment in the laboratory are the same as those in the open to-day: and *a fortiori*, how about those that influenced the early ancestry? Experiments reveal present potentialities: but it cannot rightly be assumed that they reflect trustworthy facts of history.

It must be left for others to pursue such analysis in an ascending scale towards the more advanced vegetation of the land, with its multiplicity of types and their high specialisation. There we may expect to find the traces of early evolutionary steps disguised and overgrown by later adaptive change: as are those of the Coenopterids in the highly elaborated organisation of the Leptosporangiate ferns. But notwithstanding this, as affecting the vegetative system, the spore-producing parts, from which theoretically the whole of that interpolated system originated, retain their character: and of these the micro-sporangia show a degree of conservatism, which links the organisation of the whole series historically together more effectively than any other feature of land vegetation. We conclude that, in a widely comparative rather than a phyletic sense, such sporangia as the pollen-sacs of the flowering plants are the correlatives of the sporangia of the primitive Pteridophytes, and finally of the distal capsule of some simple type of telome resembling that of the liverworts and mosses.

Within the last sixty years the age of morphological guesses and preconceptions should have passed away. Even cold formal comparison of plants, living and fossil, is outworn: unless it be related to biological probability. The attack should now be along lines of reasoning based on function, carried out inductively from simpler types upwards, and aided or checked as the case may be by palaeontological fact. Unfortunately neither the demise of an age of guesses, nor the birth of an age of inductive method upwards has yet been universally accepted as ruling the practice of current morphology.

References for Chapter XI

Chapter XI has been written in a more technical style than the rest. Its object has been to give a comprehensive view of the changes of outlook on the vegetation of the land during the period since 1875. But the material facts, especially those relating to the higher plants, are not as yet in hand, upon which this can be carried to a definite conclusion: and the opinions expressed must be held as provisional. A short bibliography is here given, so as to lend stability to the historical sketch which the chapter presents.

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CHAPTER XII

PAST—PRESENT—FUTURE

A Summary

In the foregoing chapters I have tried to sketch the changes of botanical scene brought about in the 'seventies of the last century, as they struck the eye of a participant. In doing so the persons who promoted them have been introduced, and their several shares assigned to each of them. It remains to place those events in relation to the broad stream of scientific progress, and to touch upon the secondary effects by which botanical science now impinges in increasing degree upon modern life: expanding its amenities, and tending ever more effectively to supply its needs.

Needs, such as that of medicine, have remained fundamentally the same throughout history: as is shown by records which survive in or about our ancient monasteries and universities. For instance, in the fine Jacobean Hall close to Fountains Abbey, and actually built in the early seventeenth century from its consecrated stones, there have been collected into a museum many relics of the old foundation; artistic, domestic, and ecclesiological. Documents of the highest interest are also to be seen there, relating to the ancient monastery. One of these has a particular bearing on the science of medicine in mediaeval times. It is a thick volume of manuscript written on vellum, comprising a collection of medical works by Bernard de Gordon, Professor of Medicine in the University of Montpellier, *circ.* 1285-1320. Doubtless it was held as a standard text in the library of the abbey : for the main part of the book consists of his best-known work, *The Lily of Medicine*, which was popular throughout the fourteenth, and was

several times printed in the fifteenth and sixteenth centuries. With it are also bound up certain smaller works, and particularly a *Thesaurus Pauperum*, probably by the same author. It is a medical handbook for those who could not afford to pay a doctor. In this collective volume we may see an authentic link between the practice of medicine derived from antiquity and the manuscripts of the mediaeval church, and through them finally to the printed page.

It was upon the broad back of medicine, so essential a factor in the advance of any social state, that a general knowledge of plants was borne onwards from the classical period to later times. Manuscripts, such as that in the museum at Fountains, are apt to survive and tell their tale, while the abbeys they served now stand as roofless ruins. A catalogue of those commodities economically of use to man, and of those simple remedies which were by classical tradition derived from plants, became part of the equipment of any learned centre, and particularly of the religious houses scattered through country districts. With the dissolution of the monasteries this learning was in danger of being lost. It was, however, maintained in the universities where chairs were founded and botanic gardens instituted, such as that at Oxford modelled on the Italian style. Elsewhere this learning was apt to slip away into other hands. Doubtless the trade of the apothecary and of Shakespeare's 'wise woman', based on primitive remedial lore, expanded as a consequence: while an impetus was probably given to the rise of that age of herbals that followed the dissolution of the monasteries.

Later on, in the seventeenth century, there came a period of more exact observation. Detailed curiosity was aroused by the introduction of the compound microscope. Its direction upon the tissues of plants resulted in those surprisingly accurate revelations of structure submitted about 1680 to the new-born Royal Society, by Grew and Malpighi, who were the founders of modern plant-anatomy. Others upon the continent followed

their lead, and a fair knowledge of the structural skeleton of the plant was gradually built up throughout the eighteenth century. But it was not till the early nineteenth century that protoplasm itself was recognised by Von Mohl as the living body, and the nucleus detected by Robert Brown as a constant feature within it. All this, together with a considerable amount of experimental physiology in which our own Hales and Knight played a conspicuous part, and with much surmising by Goethe and others as to the underlying foundations of form in plants, was drawn within the frame of the text-books of the middle of the nineteenth century. Particularly we may specify that of Sachs. Each of its successive editions took a place as the standard work of its class for Europe. But the detailed systematic treatment of flowering plants found no footing there. With this apart, the science of botany was, at the period which claims our interest here, in such a state that its essentials could be treated as a unit, and comprised in a single volume. It is this which has been designated by F. F. Blackman as the general solution of mixed botanical knowledge. Sachs himself has been described as the last if its 'epitomists'.

Side by side with this more generalised study, though dating back itself to much earlier days, was ranged the classification of plants. It grew up in response to an imperative call for method in recording the increasing number of recognised forms which vegetation presents. We need do no more here than allude to the successive phases of systematic botany: first, the simple list of types distinguished from one another: then the reasoned catalogue based on similarity: and last their methodical seriation leading up to a presumption of affinity. Those specific types which showed the greatest likeness were grouped by common consent into genera, each species being designated by Linnaeus under its generic and specific name. The genera again were grouped into cohorts and natural orders. The cohorts, as Sir Joseph Hooker has himself explained in the *Genera Plantarum*, were intended to link the relationships of the orders to one another. Various attempts at

codification of such grouping have from time to time been put forward: but the nearest approach to finality at the date of the revival of the 'seventies was that in the *Genera Plantarum*, by Bentham and Hooker. Its first volume appeared in 1862, and the last in 1885. Its production thus ran parallel with the repeated editions of Sachs's text-book. The latter may be held as successive codifications, each valid up to its own date, of the organography and physiology of plants: while the *Genera Plantarum* summarised the results of the systematic study of the higher plants. In respect of both of these the *Origin of Species*, published in 1858, had already offered hypothetical interpretation of the facts according to a theory of evolution.

Adverting to my own story in my early years at school, when the interest in botany first gripped me, I did not realise the great change that was so soon to come over its study in Britain, as also in the United States of America. Later I gradually awoke to the excitement of the age. On first entering the university I found myself torn by a divided allegiance between the old systematic and the newer biological discipline: but very soon I allied myself firmly with the new. The fact was that in the middle of the nineteenth century two alternative disciplines were developing in the academic study of plants, and already in 1875 their difference was clearly reflected in the current literature. They were, it is true, fundamentally related, and the doctrine of descent would be applicable to each. But at the time they were as distinct in spirit and in practice as they were in the geographic localisation of their pursuit. The structural and functional method of approaching the study of plants and of their life-histories had been specially developed in the universities of the continent, and particularly in those of Germany. Under the systematic method the natural relations of flowering plants in the adult state occupied the attention of the leading botanists in Britain, almost to the exclusion of other phases of the science: and this exclusiveness characterised the work then being done in the British universities. As we have seen,

detailed systematic work found no place in Sachs's text-book, nor did the biological study of plants invade the austere pages of the *Genera Plantarum*. Few at that time held a balance between both: but Sir Joseph Hooker, like a Colossus, had a foot down in either camp. Such was the state of botanical affairs respectively on the continent and in Britain in 1875. It was reflected not only by the printed page, but also in the class-room.

It was in the latter that Thiselton-Dyer initiated the revolution in the academic teaching of botany in Britain, by inaugurating his practical classes for the study of the living plant at South Kensington. They were given in Huxley's laboratories, and the work was modelled on his method. Thereby Thiselton-Dyer attempted to level the balance between the two disciplines, which had dipped dangerously in Britain in favour of purely systematic study. It was a difficult task, and as a matter of fact the change brought about in 1875 was somewhat overdone. The systematic treatment of flowering plants, though not wholly neglected by the followers of the 'new botany', sank towards a subsidiary place in the new curriculum: and we grew up with a very deficient knowledge of phanerogams, beyond the microscopic structure and life-history of some of them, together with the leading features of a few selected natural orders. I remember about 1876 how I longed for a train of wagons to convey the Cambridge herbarium away to Kew, and so to vacate for the new botany the rooms that would have served its needs. A crude idea no doubt, but it reflected the inverted narrowness of outlook which the time had imposed upon us. There was in fact a large element of truth in Sir Joseph Hooker's complaint, that we did not know our plants. In the sense of the real systematist we did not, as any one soon learned who at that time passed direct from the university into one of the great herbaria. But on the other hand, we of the new school of botany did look out upon the whole vegetable kingdom as consisting of living things: a knowledge of their vitality, and of its consequences was the chief end at which we aimed.

The immediate result of the change of discipline and effort introduced at South Kensington was to shepherd students of botany in Britain towards the general solution of mixed botanical knowledge as conveyed by Sachs's text-book, which appeared in its English translation of 1875. The courses of instruction were planned so as to give a conspectus of the vegetable kingdom as a whole, not only of the flowering plants: and to follow through the life-history of those that were selected, so far as possible in the living state. In order to bring such a scheme within the time-limit of any ordinary student for a degree it was necessary to focus attention on definite examples, rather than to cast a superficial glance over the whole. This led to the adoption of Huxley's method of teaching through a limited number of types. His scheme worked well in the hands of men like Huxley himself who could advance outwards, with knowledge and personal experience, from a given central type towards general conceptions as to related organisms. But it suffered some discredit from misuse by less gifted teachers. Nevertheless even these could not annul the essential feature of Huxley's method, adopted also by Thiselton-Dyer. This consisted in personal experiment and observation by the student himself, with intelligent reasoning based upon his results.

The effect of this was to bring out the individual tastes of those so trained, and the better students of this British revival were not slow to exercise such choice of special study as was open to them. Long before 1875 the major branches of the science had been defined as anatomy, morphology, physiology, and systematic and economic botany. Even the study of fossil plants was already in existence, though its pursuit lay as yet in the hands of geologists rather than of botanists. But the period when the change of discipline came in Britain was a time of promise for the botanical world at large rather than of realisation. The science, whether at home or abroad, was then like a bomb that bore within its compass many special lines of research, ready to diverge and even to

separate. The prospect of extension of knowledge along one or another of these lines was fascinating to any alert student, and his zeal was stimulated by the encouragement and suggestion of the teacher, so soon as any individual having the necessary opportunity and mental equipment should present himself.

A feature of the revival which bore special fruit was that, in place of the introspective and insular tendencies of earlier years, the botanical outlook from Britain tended now to become international. We all sought to better ourselves in the knowledge of foreign languages, especially German. Few public-school boys of the 'seventies had any knowledge of this language when they entered university class-rooms. Lecturers in the natural sciences might recount what they had themselves extracted from foreign literature, while translations beginning to issue from the Oxford Press greatly aided us. But this proved not enough for the young investigators, who aimed at tapping the sources for themselves. Clearly the most effective means to this end would be to visit foreign laboratories, and while learning botany from the professor of our choice to acquire the language of the country at the same time. This most of us did. The effect was that British botany has since taken a better international position than before 1875: a fact that greatly strengthened the revival. A silent witness to the reality of this is seen in the fifty volumes of the *Annals of Botany*, and their comparison with those of any botanical production of earlier date in Britain.

During the decade 1875-1885 the usual curriculum of mixed botanical knowledge remained undifferentiated in Britain, though with separate senior and junior classes. In point of personal teaching the professor himself was supposed to cover all the nascent branches, however definite might be his own choice of a specialty. But as the science expanded this condition could not be expected to last. The first clear departure from it appeared in Cambridge. The appointment of Vines as reader in 1883 suggested the emergence of special teaching in physiology. This became

more real in 1884, when Francis Darwin was appointed university lecturer, and later succeeded Vines in the readership. A sub-department of plant physiology in its pure form then came into being. A like process, determined in method and date by local conditions, has followed elsewhere in the larger botanical centres.

Towards the close of the century other lines of specialisation emerged from within, commonly as a consequence of the discovery of new methods, or of new facts and new syntheses based on those facts. Frequently their origin may be associated with the work of some individual which has stimulated that of others, or even created a new school of thought or observation. The development of botanical study as a whole might be compared with the progress of a flock of sheep, advancing fanlike over a large plain, but with irregularities of formation defined either by individual enterprise or by the varying richness of the pasture: or by both. Those who follow the history of botanical science will readily find instances of this from the years following 1875. They will note the advance of anatomy that found its origin in the stelar theory of Van Tieghem: the widening of physiological view that followed the demonstration of protoplasmic continuity: the closer alliance of physiology and systematic treatment that resulted from the ecology of Warming: the stimulus to comparative embryology that sprang from Treub's researches on the Lycopods: the wide repercussions that followed Strasburger's statement on periodic reduction: and the influence of his cytological work, and of Mendel's complementary experiments, on the problem of heredity: the effect of de Bary's studies of infective disease on the pathological side of agriculture and horticulture: or, lastly, the impetus given to fossil botany by the pioneer work of Williamson and by the later discoveries of Scott, Kidston, Halle, and Kräusel. Each of these events was apt to fire fresh enthusiasm and to establish a vogue for some special branch of enquiry, tending to advance it to the outer fringe of prior knowledge, and beyond it.

This by no means exhausts the tale of those special developments of the science which have marked the period immediately following the revival of botanical study in Britain. Expansion of such interests was then in the air, and the British revival itself may be held as only a part of a general movement. We now see from the distant view of half a century that the reformation in Britain was happily timed: shall we say rather that in 1875 a general renaissance was imminent, or perhaps actually in progress on the continent, and somewhat ahead of that at home? Such a time proved favourable for the germination of the seed sown at South Kensington. We need not expect any repetition of such synchronised events. It is highly improbable that any period of botanical torpor like that of the 'sixties and early 'seventies will ever recur in our universities: nor can we expect future conditions to replace that atmosphere of rebound that followed the revival of evolutionary theory, bearing with it as a lesser and later result the recovery of tone in British botany. A powerful preventive of such recurrence lies not only in the widening differentiation of the many branches of botanical science, but also in the close nexus between their most advanced fringes and kindred sciences, such as physics, chemistry, physiology, zoology, and geology. Moreover, the application of some branch or another of botany to almost all the modern developments of social life makes a continued pursuit of the basic science imperative. Even the proverbial man in the street now feels his own dependence upon the vegetable kingdom for the usual amenities of modern life in quite a different way from his predecessors of the last century.

So we may confidently expect that further developments in botanical study, whether by purely scientific research or by application of its results to modern requirements, will extend in the future as in the immediate past. The flock of investigators will increase in numbers, and will spread outwards, led as before by individual originality, or stimulated by opportunity. But as the fan spreads there will be an ever-growing risk of marginal dis-

integration of the flocks: extreme branches of specialisation tending to lose touch with one another, and even with the central science itself. There are also signs of an irresponsible factor leading towards lop-sidedness which may best be called mere fashion. The progress of a science is not as a rule equable; it moves by fits and starts. Sometimes, as we have seen, these are dictated by new discovery of fact, sometimes by new synthesis of facts already known. The herd-sense may thus be aroused among its votaries, leading to some definite though perhaps isolated advance. For a time some special branch may thus become prominent: but the interest in it will be liable to fade as difficulties of further observation, dearth of facts, or exhaustion of the new vision resists and weakens further progress. The fashion then wears out, and dies of inanition. This is no fancy sketch: instances of it occur in the history of any progressive science. At the moment there is in botany a definite bias towards topics on the border-line of physics, chemistry, and physiology: while cytology, genetics, and palaeobotany are also in the ascendant. Meanwhile the old fundamental branches appear to be more static. Particularly is this the case with morphology, not only in Britain but also abroad, as Von Goebel himself has noted. It is somnolent, suffering from an ill-digested surfeit of fact.

Botanists are at the moment more concerned with process as shown in function, than they are with record as embodied in form. But statistics which the first of them would supply cannot replace history. The historical, that is the scientific, study of form should be built up from below, as a record of the action of those factors that affect form, whether inherited or more directly produced. The true inwardness of the study of morphology lies in ascertaining how plants, as we see them in all their varied contours, came to be such as they now are. From this point of view each detailed demonstration of functional process carries its own value, though it is not to be held as an end in itself. Each is rather a step towards the solution of a larger problem of form. Under

the name of 'Morphology' the study of this problem was originally based on some ideal type of plant, with parts such as are seen in flowering plants. It soon lost itself in the fruitless discussions of nature philosophy: even the rugged influence of Schleiden did not suffice to guide morphological thought back into the channel of induction, though this had been clearly defined long before by Bacon.

The brilliant discoveries of Hofmeister among the Archegoniatae brought the remedy of vision from below. They were joyfully adopted by Sachs in his *History of Botany*: he saw with the eyes of an optimist how Darwin's theory of descent had only to accept what Hofmeister's genetic morphology had actually brought to view. Thus eventually in 1875 a scientific morphology seemed to be already replacing the poetic 'ideology' of Goethe. This was the year when Sachs's text-book was first published in English, thus synchronising with our British revival. There followed a period of general enquiry into life-histories, which as regards the Archegoniatae has now approached its completion. At the present moment morphology seems to be waking up again after a period of marking time. What is now required is evidence of causality ruling the various mouldings, external and internal, as seen on comparison of plants at large. For this the solution of many problems of process will be needed: intimately physiological, cytological, genetic, and hereditary. The 'how' and the 'wherefore' must be better understood before their collective record in form can be rightly read. This is the aid which morphology hopes to receive from physiology. On the other hand, positive facts are pouring in from palaeontological sources.

Meanwhile a critical question for the morphologist of the future will be whether all the leading factors of causality have yet been detected. Any further factor affecting form that has escaped notice, or has not been duly appraised, will strengthen his position. It must suffice here to mention the future that appears to be rising before the study of hormones and auxines. But their observed

effect on growth only carries the question one step further back, to that of their own origin and localisation. Another example is seen in the factor of size, which has not yet been duly valued in its effect on primary form in plants, as distinguished from those results of secondary change which tend to disguise and overgrow the primary features. Measurements and other observations taken from the parts of primary development, as seen in primitive land plants, have shown that a widely spread relation exists between size and form, which only the blind can miss, once the mouldings have been compared and the figures stated. Such relations do not arise by chance. We are bound to recognise this size factor, which suggests some definite causality, though the immediate source of its effectiveness may still be an open question. The general omission of the factor of size, and of the data ruling the localisation of hormones, in earlier discussions of form, shows the imperfection of causal analysis hitherto current. Its focus for the future will lie in study of the growing point itself, where primary form originates.

It is no wonder that logically-minded students have been puzzled by the changes of attitude, and the omissions of the factors cited, in the past study of form. Hence they will be inclined to stand aloof from morphology, giving preference to other branches of the science, the pursuit of which appears to promise more cogent arguments and more direct results. Before the arrested interest can be again secured morphology will have to begin rebuilding from below, upon a basis of causality in evolution, and aided more freely than hitherto, or perhaps checked, by measurement, by experiment, and by comparison conducted in accordance with palaeontological fact.

Returning from this excursion into the future of the science in its morphological aspect, we may consider certain points of its internal organisation, which will become more and more critical as the subject expands, and its application to modern life extends. Every botanical department centres round an official head,

and its success depends greatly on his qualifications for that position. Without venturing to dictate to others, there can be no harm done by expressing opinions based on long experience during a time of active development. A professor should be something more than an ultra-specialist. An ideal for the future may be one having an all-round knowledge of the subject, but with some definite line of enquiry peculiarly his own. For there is no stronger stimulus to the student than feeling that through his official leader he is in touch with the outer fringe of knowledge, with glimpses into the unknown. Skill and experience in the art of teaching and practical demonstration are indispensible, though often neglected. Another essential qualification is administrative ability, combined if possible with varied experience, acquired preferably through foreign travel. Finally, the most essential qualification of all is personal sympathy with the learner, in his difficulties and enthusiasms. Such qualifications are not easily to be found centred in one person. Among the rarest at the present time is that of all-round experience: for commonly a preference for a special line of research is defined early, and the first successes in publication tend to circumscribe the personal interest. Such successes often prove a stronger credential with an appointing board than ability to administer, or even than didactic skill. Thus we may see reflected back to those with whom appointments rest, and indirectly to their advisers, the responsibility for selection with an eye to a well-balanced future. But if selection follows lines such as these here specified, there will be reason to hope that there shall never recur that restricted outlook on the practice of the science which characterised the period before the renaissance of the 'seventies of last century.

The growth of departmental life has been a natural consequence of the differentiation of special branches of the science, and of the correlative enlargement of staffs. The departmental head need not himself try to keep abreast of all the branches: only the strongest could bear that burden to-day, and still remain an

active investigator, as an example to the rest. He should, on the other hand, keep a careful watch over specialist's myopia, whether in himself or in his juniors. A natural corrective against this lies in social intercourse with his colleagues elsewhere: through the medium of learned societies, or of the British Association. But within the narrower sphere of his department he may establish near relations with the members of his own junior staff and research students. This is readily stimulated by informal meals in the laboratory, that make the long working days practicable. Such daily meetings lead not only towards sociability, but also to that pooling of results and ideas which proves the best antidote to over-specialisation in any one branch. Before the turn of the century this departmental life hardly existed. Its encouragement to-day tends to ward off any drift towards an unbalanced outlook, such as that which ruled in Britain before 1875.

Against this risk there is also that safety which follows from numbers. In 1875 the British centres where botany was officially taught totalled hardly a dozen. As measured by attendance at the British Association—even at the Jubilee Meeting at York in 1881 at which I was present—the botanists were a mere handful, submerged in the still undivided section of biology. But ever since the foundation in 1895 of a separate section for botany their numbers have grown, till now the annual attendance at section K has passed well into three figures. In the International Address Book of Botanists there are about a thousand British names, and most of these are of persons employed professionally. When we consider such numbers, and the varied ways in which the results of their research are now applied to the ordinary affairs of life, any return to the narrow outlook and practice of 1875 seems impossible.

I cannot do better than conclude with the very convincing words of Mr. Walter Elliot, used by him on September 21, 1936: when, as Minister of Agriculture, he welcomed the Delegates to the British Commonwealth Conference in London. He recalled

how eight Bureaux had been formed, one for each branch of agricultural research. He specified as an example of the result the great benefit that had accrued to producers, both at home and overseas, from the low-temperature work of the Food Investigation Board, at Cambridge and at East Malling. It is the task, he said, of the Agricultural Bureaux to give to administrators, politicians, and leaders in the Government, a head of steam to work on. With botanical research thus officially linked with imperial interests its future is assured. Moreover in the pure science of Botany, the advance of which necessarily precedes its application for the public good, we may now count on the support of any enlightened Government: for it is the fire of pure science that generates the head of steam demanded by the Minister of Agriculture.

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